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A locomotive hospital. Swinging a mammoth locomotive by powerful electric cranes

THE KEY TO THE RAILROAD SITUATION [See page 344]

The Age and Area Law*

A Fundamental Law of Geographical Distribution

By James Small, M.Sc. (Lond.), Ph.C.

Assistant Lecturer in Botany, Bedford College, and Lecturer in Botany to the Pharmaceutical Society of Great Britain

A THEORY of geographical distribution has been brought forward in a series of recent papers by Dr. J. C. Willis, which should radically change the outlook on many problems of evolution. It has been expressed variously by the author at different stages in the controversy which has arisen as a result of his work.

The law was first stated in the following form: "The commonness of a species depends upon its age from the time of its arrival in, or evolution in, the country.[1] The commonness of any individual species will, of course, depend upon its degree of adaptation to local conditions and upon many things, such as the sudden appearance of new factors like diseases, which can only be regarded as chance." As the result of further study and some controversy, it was restated later thus: "The geographical distribution of a species (*i. e.*, the area which it includes within its outer localities) within a fairly uniform country not broken by serious barriers depends upon the age of that species within that country." As a result of further controversy, the law is now stated to be subject to the qualifying phrase "so long as conditions remain constant," and the following causes are enumerated which may modify the operation of the fundamental principal of age and area:

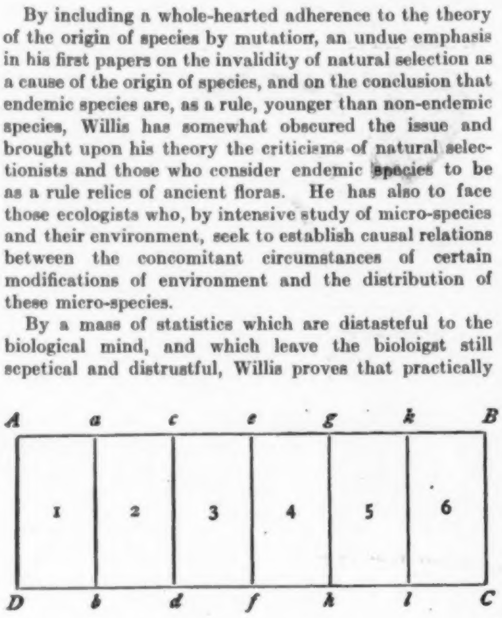
- Chance (the operation of causes as yet not understood);
- Action of man in opening up a country, cutting of forest, exploring, making fires, etc., etc.;
- Interposition of barriers, such as mountains, broad rivers, deserts, arms of the sea, sudden changes of climate from one district to the next, and the like;
- Geological changes, especially if involving change of climate;
- Serious changes of climate;
- Natural selection;[2]
- Local adaptation (a species may have a peculiarity which is useful in one country and valueless in another);
- Dying out of occasional old species;
- Arrival of a species at its climatic limit;
- Density of vegetation upon the ground at the time of arrival of a species;
- Presence or absence of mountain-chains in the land over which the species has to travel in arriving;
- Relative width of union between the country of departure and that of arrival (the wider it is the more rapid may be the spread of the species in the new country); and so on.

It is recognized that this list is not complete, but it is claimed that, although some of these causes probably come into action in almost every case of any one species, the total result is not a differentiating action, and the different effects of these various modifying causes acting in so many different directions cancel out when large numbers of species are dealt with. The remaining effect after the cancellation is shown to be due to some mechanical cause, such as age, which acts on all species, genera and families alike, and which is, "at any rate, independent of morphological and biological qualities."

Willis founded his hypothesis on a large mass of statistics of the estimated distribution of species in Ceylon, but has confirmed it as a law by dealing with the measured distribution of species in New Zealand. At present, although recent researches extend the operation of the law to the lower plants and (unpublished work) to some groups of animals, most of the facts are drawn from the known distribution of the flowering plants, chiefly in Ceylon and New Zealand. Dr. Willis's chief point lies in the emphasis he lays upon the obvious commonplace that, as a rule, if a species is not dying out, the longer it exists in any given country the wider will be its distribution in that country. It will be conceded by most botanists that, as a rule, the species of Angiosperms are not dying out, and, as the law of age and area applies only as a fundamental, liable to be altered by a variety of causes in particular instances, it follows that it applies, *as a rule*, to Angiospermous species. It must be remembered that there are numerous exceptions to the law of gravity and to most other fundamental laws, and these are explained by modifying causes. The law of gravity is not considered disproved because a balloon or an aeroplane rises.

By including a whole-hearted adherence to the theory of the origin of species by mutation, an undue emphasis in his first papers on the invalidity of natural selection as a cause of the origin of species, and on the conclusion that endemic species are, as a rule, younger than non-endemic species, Willis has somewhat obscured the issue and brought upon his theory the criticisms of natural selectionists and those who consider endemic species to be as a rule relics of ancient floras. He has also to face those ecologists who, by intensive study of micro-species and their environment, seek to establish causal relations between the concomitant circumstances of certain modifications of environment and the distribution of these micro-species.

By a mass of statistics which are distasteful to the biological mind, and which leave the bioloigst still sceptical and distrustful, Willis proves that practically



all genera and all families have the same type of distribution. It is, however, unnecessary, except as the last stage of the proof, to put the facts in a statistical form. If the area *ABCD*, Fig. 1, be any given region into which species arrive from outside regions at *AD*, then the first group of species to arrive would have spread and occupied the area *ABCD* if a sufficiently long interval had elapsed between the time of their arrival and the time of the analysis of the flora, and if no great barrier such as a mountain-range or great river intersected the area. Similarly, the second group of arrivals would extend over the area *AklD* and so on. The most recent arrivals would naturally occupy the area *AabD*. Now, since the several areas all overlap in the area *AabD*, *that region should have the greatest number of non-endemic species, i. e., species which also occur outside ABCD, and the number should decrease regularly to a minimum in kBCl.*

If, instead of arriving from outside regions, the species were evolved within the region, not by a differentiating cause such as natural selection but by chance mutation, then, since the greatest number of species occurs in *AabD*, there will be the greatest chance of species arising in this region. If the laws of chance govern the origin of species *the number of species which are endemic, i. e., confined to ABCD, will be greatest in the area AabD, and the numbers should decrease regularly to a minimum in kBCl.* New Zealand is an area somewhat similar to *ABCD*, and Willis has proved that the two predictions in italics hold good for the distribution of species in these islands. For the sake of simplicity, the point of arrival has been placed at the end, instead of somewhere in the region *df*, as it is in New Zealand. It appears probable, therefore, that the laws of chance do actually govern the origin of species, and that the species spread more or less mechanically as a rule.

Considering further the number of non-endemic species occupying the six areas, 1, 1-2, 1-3, 1-4, 1-5, 1-6, on the age and area hypothesis a species which occupies only area 1 or *AabD* is a more recent arrival than one which occupies the whole area *ABCD*, and the other species occupying the areas intermediate in size are intermediate in age (in the country). If the region *ABCD* was connected with the source of its non-endemic flora a long time ago (geologically), many of the species will have had time to occupy the whole area, and the number of non-endemic species *limited to the area AabD*[3] will be smaller than that of the species occupying the whole area *ABCD* (supposing the rate of immigration to have been more or less constant) and the number of species occupying intermediate areas will be intermediate also, forming a decreasing series from *ABCD* or 6 unit areas to *AabD* or 1 unit area. This has been found to be the case for both Ceylon and New Zealand.

Considering now the case of the endemic species, there is no reason to suppose that new species have ceased to be produced so that we can postulate a relatively constant supply. If the laws of chance govern the origin of species we can picture the production of new species from an immigrant at a constant rate in the following way: while occupying area 1, it gives a species, α, while occupying areas 1 and 2 it gives a species, β, in area 1 and another, γ, in area 2, and so on, until, while occupying the areas 1-6, it gives 6 species. Species α will meantime have spread (possibly over areas 1-5) and so will β and γ and other intermediate species to a smaller degree. There will, therefore, be one endemic species occupying 5 unit areas, two occupying 4 unit areas, three occupying 3 unit areas, four occupying 2 unit areas, and five occupying 1 unit area, while six will be just beginning to spread. From this it will be seen that the number of endemic species occupying a small area is naturally larger than the number occupying a large area, while the intermediate areas possess intermediate numbers of endemic species forming an increasing series from 5 unit areas to 1 unit area.[4] This is exactly the opposite of what occurs with the non-endemic species (see above) and the gradation in opposite directions of the numbers of species occupying the various sizes of areas is exactly what Willis finds by comparing the endemic and non-endemic floras of Ceylon and of New Zealand.

Among the many predictions made and confirmed by Willis about the flora of New Zealand were three which may be given thus: if the area *ABCD* was separated from the source of its non-endemic flora at a much earlier period than a similar area *A'B'C'D'*, then the average area occupied by the species in *ABCD* would be greater than the average area occupied by those in *A'B'C'D'*. This would be the result of more non-endemic species having had time to spread and occupy most of the areas 1 to 6 in *ABCD*, while new species would not arrive sufficiently rapidly to give proportionate increase in the number of species occupying only area 1 or areas 1 and 2. Given that the rate of spreading of a species is rapid compared with the rate of origin of new species, the endemics as well as the non-endemics will show a larger proportion of widely distributed species in *ABCD* than in *A'B'C'D'*. By comparing the average area occupied by the species in Ceylon and in New Zealand and the numbers of widely distributed non-endemic and endemic species in these two islands, Willis has confirmed these predictions.

Another prediction made with the aid of the age and area law and verified in the New Zealand flora is that if the species enter *ABCD* at *f* instead of at *AD*, then if the area of distribution of any one species extends to the line *cd* it will probably extend to *gh* also, if to *ab* then to *kl* also, etc. Similarly with endemic species, those produced early in the history of the flora would extend over the greatest area; being produced early they would have arisen near *f*, so that if the area occupied by an endemic species includes area 1 or area 6, the whole area of the species is likely to be large. This also has been verified.

Therefore, the age and area theory, having been subjected to the test of prediction and verification, may be taken as proved, and its universal application subject to modifying causes being proved by published and unpublished work it remains no longer a theory, and must be regarded as a law.

It is necessary to point out that even with the age and area law there may be endemic species which are older (in the country) than some recent arrivals. For instance, an endemic may be old enough to spread all over *ABCD*, while an immigrant may be so recently arrived that it has spread only over *AabD*. This, however, does not interfere with the general statement that the number of endemic species increases with their rarity, while the number of non-endemic species decreases with their rarity, nor with the conclusion that, therefore, the endemic element in a flora is, on the whole, a younger element than the immigrant element. Students of endemism are apt to concentrate their attention on the peculiar relic endemics (which undoubtedly exist) thus producing the impression that the whole endemic flora is composed of such species.

This part of Willis's work has recently been criticised very shrewdly by Sinnott, who gives statistics of the growth form of the species (trees, shrubs, and herbs) of Ceylon and Peninsular India, which prove that the woody forms, especially trees, are much more abundant in the endemic than in the non-endemic flora. He also cites

*From *Science Progress*.

[1] As this applies to the average species, and not necessarily to any particular one, the species must be taken in groups of about 20.

[2] Natural selection enters to a large extent into the determination of the commonness of a species within its area of occupation.

[3] This is to be distinguished carefully from the tota. number of species in the area *AabD*.

[4] The actual area might be *AabD*, *abdc*, *cdfe*, *efhg*, or *ghlk*.

similar circumstances in a number of small islands and Australia. The predominance of woody forms in insular endemic floras has been remarked upon by all those who have dealt with the subject, but whether this is due to these floras being ancient, as Sinnott and others suggest, or whether it is due, in part at least, to some specific effect of insularity, remains to be decided. It is more than probable also that herbaceous species more frequently possess greater powers of dispersal and would reach isolated areas more readily than the arborescent species, thus raising the percentage of herbaceous forms in the non-endemic flora, but in no way invalidating the age and area law, which is applied by Willis only to age within the given region. Another objection raised by Sinnott to the age and area law is that "it necessarily implies a greater antiquity for the herbaceous than for the woody vegetation of the earth." The age and area law deals, however, only with age within the country, not with absolute age except in the case of endemics.

Sinnott, however, admits more than Willis maintains when he says that "there is doubtless much truth in Willis's main contention that, other things being equal, the longer a species lives, the wider the range it will cover. The chief argument on which the hypothesis is based is the fact, which in the face of the data presented cannot well be doubted, that endemic types have comparatively narrow ranges and non-endemic types comparatively wide ones." In fact, his point of view is summed up in his concluding sentence where, after mentioning one or two additions to the list of "modifying causes" of the age and area law in relation to endemism, he says, "The purpose of the present paper is to point out certain of these complexities and to show that no single hypothesis like that of 'age and area,' however valuable it may be in explaining certain facts, can be used as a key to the whole problem."

The hypothesis that endemics are as a rule recent receives confirmation in a recent contribution by Taylor, who, after an examination of the endemic flora of the vicinity of New York, concludes that while fourteen of these species are the result of specific or generic instability (*i. e.*, recent mutations), two have arisen by adaptation of wider spread species to the local environment, and only five by the dying out of previously more widely distributed species.

The mutation controversy is involved, but at present only one contributor, Ridley, has attempted to uphold the theory of natural selection to the exclusion of mutations. Ridley criticizes the age and area hypothesis on various grounds. He takes the very common species of Willis and Trimen to mean species which are abundantly represented by individuals instead of species which are widespread, and gives various cases of such locally abundant species which have died out in particular localities, most especially as the result of the advent of man.

He claims to prove that these species may disappear without a geological catastrophe. As he quotes volcanic action and the glacial period as non-catastrophic occurrences, it is difficult to follow his argument. It is also difficult to understand what Mr. Ridley thinks of the case for mutations as he states in his summary that "the mutation theory . . . is not in accordance with the facts," while in the text of the paper he gives a very good account of the action of natural selection in killing out injurious "mutations" and of the origin of species by mutation. "This theory," he says, "can be tested and proved by the study of mutations." He proceeds moreover, to quote well-developed spines, a change in stature from about 6 in. to 10 ft., etc., as "one or two examples of infinitesimal variations."

Willis has replied in a recent paper by acknowledging that the action of man and other subsidiary causes may modify the action of the law of age and area. "Mr. Ridley quotes about seventy cases in various connections. Many of these, *e. g.*, those on p. 555, are excellent illustrations of what I have said that a very small accident may kill out a species in the class VR." "VR" means very rare, and among the cases mentioned is that of *Didymocarpus Perdita*, Ridley. The note on this species by Ridley is, "I found two plants of this on a bank in the center of Singapore surrounded by extensive cultivation. It has never been seen again." It seems not impossible that the description of this species by its author involved its destruction an excellent example for the arguments of both Ridley and Willis that incipient species are easily killed out. Willis, however, "for the sake of argument," gives all Ridley's objections concerning the distribution of these seventy species their *maximum* effect, and shows that the resulting corrections of the figures for the distribution as a whole leaves the case for age and area as strong as before.

In a still more recent contribution Willis gives five further examples of the action of age and area. Taking the orchids of Jamaica and dividing the Islands of Jamaica and Cuba into a number of areas approximately 6¾ miles square, he shows that the endemic species as a rule occupy about 3 squares, *i. e.*, the average for all the endemics is 3 squares, while the average for the species extending only to Cuba is 4.5 squares, and for the wider-spread species 5.7 squares. It is clear, therefore, that the distribution of the orchids in Jamaica follows the age and area law.

As a pertinent example of an island flora rich in endemics he takes the flora of the Hawaiian Islands and points out that 74 out of the 149 non-endemic species occur on all the chief islands, while only 41 out of the 581 endemic species cover that area. The average area occupied by the non-endemics is about twice that of the endemic species, so that here again the age and area law is at work.

Callitris is taken as an example of a Conifer. The genus is endemic in Australia and Tasmania. One species occupies the area covered by the genus, a second group of two species is less widespread, a third group of eight species is still more local, while there are seven species which occupy very small areas. This is as near to the age and area phenomenon as is to be expected in a single genus of less than twenty species.

Two examples are given of the distribution of ferns. The endemic ferns of New Zealand cover on an average a smaller area than the non-endemic ferns, but the difference is not so marked as in the Angiosperms. A very interesting point in view of the recognized relative antiquity of the ferns is that the endemic ferns occupy on the average about twice the area occupied by the endemic Angiosperms. The endemic ferns also take no notice of Cook's Strait, while the endemic Angiosperms are markedly divided into two groups by that Strait.

The deduction is therefore made that Cook's Strait originated between the disposal of the endemic ferns and the dispersal of the endemic Angiosperms, in much the same way as it is shown to have done between the dispersal of the non-endemic and that of the endemic Angiosperms.

The same points are brought out in the fifth essay, a study of the distribution of the ferns of the Hawaiian Islands. The endemic ferns again occupy a greater area than the endemic Angiosperms and a lesser area than the non-endemic ferns. The non-endemic ferns, as in New Zealand, occupy on the average about the same area as the non-endemic Angiosperms, a fact which Willis explains by the constant arrival of non-endemic species of ferns on account of the easy dispersal of the latter by means of their spores.

Another supporter of natural selection, Copeland, maintains that age has been previously recognized as a factor in distribution, and that "mutation" is merely another word for "variation." Like Ridley, he fails entirely to discriminate between continuous and discontinuous variation. Indeed, mutation or discontinuous variation has taken so great a place in evolutionary facts that adherents of natural selection seem to accept them, unconsciously admitting the facts upon which de Vries founds his theory while nominally in opposition to the theory itself. This is obvious in the following quotation from Copeland, "Regarding myself as a confirmed adherent of the doctrine of natural selection, I do not hold it in the slightest measure *directly* responsible for the origin of any species. Species originate by variation. There is not the slightest doubt that in nearly all cases . . . variation is indiscriminate in direction."

In a series of appreciative reviews de Vries emphasizes the point that "a general cause must govern this phenomenon, a cause which is, at any rate, independent of morphological and biological qualities," and concludes that the "age and area" theory is the only one sufficiently broad to explain all the facts.

Other reviews in sympathy with the theory have appeared, one by Lotsy and another by Coulter which, if somewhat non-committal, is not in strenuous opposition like those of Ridley and Copeland. Indeed, Coulter and various other eminent American botanists are quite in sympathy with the new doctrine which will, no doubt, take its proper place in the study of geographical distribution.

The present writer in work about to be published on the evolution and geographical distribution of the Compositæ has found the age and area law very valuable indeed, confirming in the case of every tribe the phylogenetic conclusions reached in the study of the morphology and physiology of the subdivisions of that large and undoubtedly recent family.[5]

[5]Since the above was written Willis has published a further paper (*Ann. Bot.* vol. xxxi. 1917) in which he shows that the area occupied by a species in *New Zealand* increases with the number of outlying islands included in the total area of the species. A most remarkable confirmation of a prediction to that effect. A further example of the action of the age and area law has been found in the Australian grasses by Breakwell.

Mixtures of Alcohol with Various Other Liquid Fuels

The method of mixed fuel has certain distinct advantages, and it is quite possible that some such method will have to be employed during the transition stage of the reform from petrol to alcohol. This method has the advantage of utilizing to the full engines and plant already in use, and it gives engineers a period of grace in which to design and manufacture efficient alcohol engines to replace ultimately the liquid fuel engines now in use. Further, it has the great advantage that it would familiarize engine makers and owners with the use of alcohol as a fuel.

English and French authorities favor the admixture of 50 per cent of coal tar benzine with alcohol. This is a method of producing a motor fuel which may be very suitable in highly industrialized countries, such as those of Europe, where there is a large production of tar, but it would lead to delay if Australia were to follow in developing this type of fuel. At the present time the quantity of coal tar benzine produced in Australia is very small.

It is certain that demands for a substitute for petrol will be very urgent before we have sufficient quantities of benzine available to consider its use for this purpose. Such benzine as is produced in Australia can be put to better uses.

A solution of acetylene gas in alcohol has been suggested, but acetyle is an expensive gas and, at present, cannot be produced in Australia without dependence on overseas importation of raw material. A patent was taken out in Russia a few years ago for a solution of hydrogen in alcohol. Hydrogen can be produced for much less than acetylene and is found to be very soluble in alcohol.

A trial of this fuel was not found satisfactory, but the trial could not be looked upon as exhaustive. A more practical application is the addition of sulphuric ether to alcohol. This idea was developed in South Africa, and a patent was taken out for it there in 1914. The fuel is called "Natalite," and consists of a mixture of about 60 per cent alcohol, 39 per cent ether, ½ per cent arsenious acid, and 1 per cent ammonia gas. This fuel was tried in an official test by the Royal Automobile Club, London, and it was reported that it was equal in every way to petrol.

It can be used in any make of petrol engine without adjustment, gives the same or a little better mileage, starts even more easily from cold than petrol, and gives a sweeter running engine and less carbon deposit. The addition of the ammonia gas is made to neutralize any acetic acid formed in the exhaust gases. This corrosive exhaust is stated to be one defect of alcohol as a fuel, but it is further stated that engines run for a long while on alcohol will show no corrosion in those parts of the engine encountered by the exhaust gases excepting in places where the gases are permitted to condense. Then, and only then, does corrosion appear. The introduction of the arsenic acid is interesting as it shows how the inventors of this fuel feel in regard to the denaturation question.

Such a fuel as "Natalite" may be of great value in the period of transition from petrol engines to alcohol engines, but its permanent use has the objection that it does not permit of such efficient use of the alcohol as can be obtained from a properly designed engine running on the spirit alone.

The method of using alcohol as a mixture is viewed with favor by such authorities as Dr. W. R. Ormandy and Prof. V. B. Lewis. The main advantages claimed for the use of such an admixture may be summarized as follows:

(*a*) It could be used efficiently in existing types of engines.

(*b*) If benzine were used, it would give a considerable impetus to the manufacture of that commodity from coal tar, and would enable supplies of alcohol to go much further than if alcohol were used alone.

(*c*) With the use of such a mixture there would be no difficulty in starting the engine from cold.

(*d*) As the use of such mixture spreads, manufacturers would so improve their carburetors and their engines that the proportion of the material with which the alcohol is mixed could be gradually reduced, and in this way the great difficulty in passing over from a type of engine in which thousands are in use to a modification thereof would be overcome

(*e*) As alcohol in the form of an admixture comes into wider use, producers would be encouraged to increase the supply of alcohol as the demand increased.—*Bulletin* of the Advisory Council of Science and Industry, Australia.

The Stone Age in America

Methods by Which the Indians Made Their Tools and Weapons

By Dr. A. F. Bonney

The discovery of ancient quarries, and in them vast quantities of stone fragments and the mauls used in breaking them, informs us that craftsmen of the Stone Age had an abundance of material from which to pick when they desired to make arrow and spear points for use in war and the chase, and knives, drills, hoes and scrapers for domestic purposes, for not only was quartzite, commonly called "flint" abundant in many parts of the country, but other material g ving the desired fracture was to be found; jasper, chalcedony, and, on the Pacific

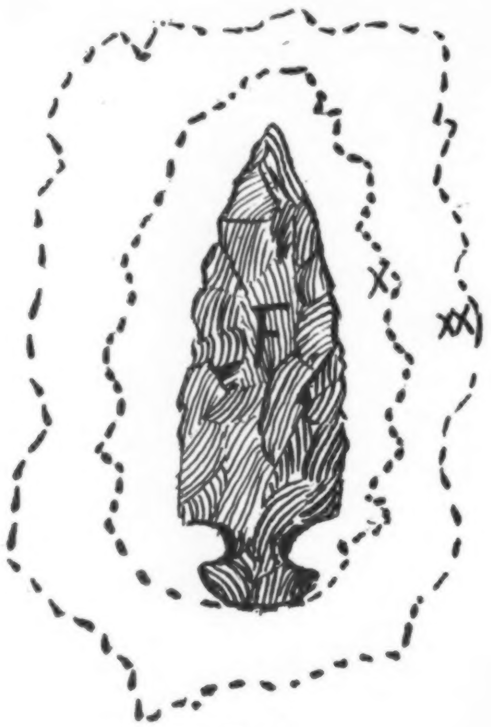


Fig. 1

coas', obsidian, while in Europe the true flint was found abundantly, which the aborigine used freshly flaked, as it was then more eas ly worked. Lacking stone to give the conchoidal fracture slate was sometimes used, and arrow points have even been found roughed out of milky quartz.

With all these advantages it is improbable that the ancient arrow maker always got the exact results he had in mind when commencing work on a flake, for no two pieces of stone break exactly alike under punch and mallet, one yielding beautiful fractures, the other breaking up like sandstone, so he soon learned to waste no time on poor material, and the large number of unfinished and broken arrow points found among the small flakes they produced so abundantly in their old-time workshops is evidence that they got probably not one perfectly finished article out of scores of flakes produced. A nodule of flint, jasper or obsidian might not yield a single piece which would be workable, as modern experimentors have probably noticed and a flake which appeared large enough to make a good, thin arrow or spear point, knife or hoe might in working be reduced to a medium sized, thick arrow point, drill or knife, or if an arrow point were broken it might be turned into a scraper, like No. 9 in Fig. 6, or a small knife, No. 15, No. 1 being too thick to be workable was with a few strokes turned into a serviceable knife.

While in Arizona a number of years ago the writer saw an old Apache Indian chipping arrow heads of quartzite. He might have been an expert, sixty or eighty years previously, but his eyes had grown dim and his hands unsteady, and the pieces he produced were rude, but good enough to be used in making arrows which he sold to tourists—after the relics had been well smoked to give them age.

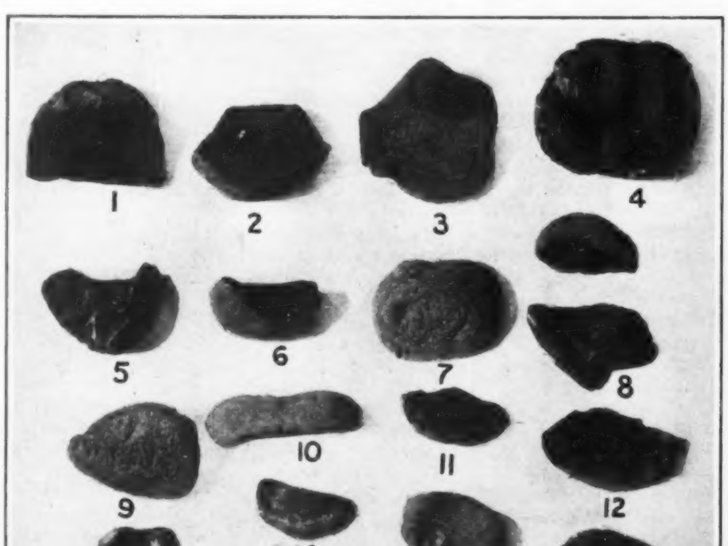


Fig. 5

The old man's knowledge must have come from the Old Time, for finding a flake to suit him, a rather flat one which was not too thick in the center, and sloped to an edge in all directions from the middle, he roughed it into a leaf shape by pounding the flat of the edge with a piece of stone. Then with a 10d nail for a punch and a billet of wood for a mallet he began working the edge, flaking first from one side and then the other, holding the punch at right angles to the surface of the stone. Then he thinned the edge by holding the punch at an obtuse angle to the surface, finally finishing it by putting the notches in the base. He worked against a thick piece of leather laid on his thigh, but had a piece of wood with cracks in it which he sometimes used to hold the flake when the edge was too thick to yield by the first method, which suggested a couple of pieces of wood in a vise for holding the work.

For chipping large pieces of stone some way must have been available for holding it when breaking off large flakes, and it is not unlikely that two persons sometimes worked together; but all smaller work was probably done by working on the knee. The punch could be set at the various angles required, while a quick, hard pressure gave the same effect as a blow from a mallet; however, as with modern mechanics, it is not unlikely that in different parts of the country arrow makers used different methods, and secured various degrees of finish.

The obsidian implements of Southern California were particularly well done, as an examination o' the collections in the Los Angeles Chamber of Commerce will show. The "bird" points found in Oregon are often called "Jewel Points" so beautifully are they executed.

The ancient craftman used his punch of bone or deer horn at three principal angles to the surface of the flake he was working according to the thickness of the edge, and it is also very likely that he soon learned to follow the line of least resistance, something vastly easier to write about than describe. These three principal angles are described in Fig. 1. A will yield a short, thick chip, B a longer and thinner one, while C, if properly applied, will give one thinner and longer than B. An examination of ancient arrowheads will show that C was used very much in thinning the bases of arrow points. On the other hand, the notches in the base were most likely done with A. A study of many old arrowheads justifies this opinion.

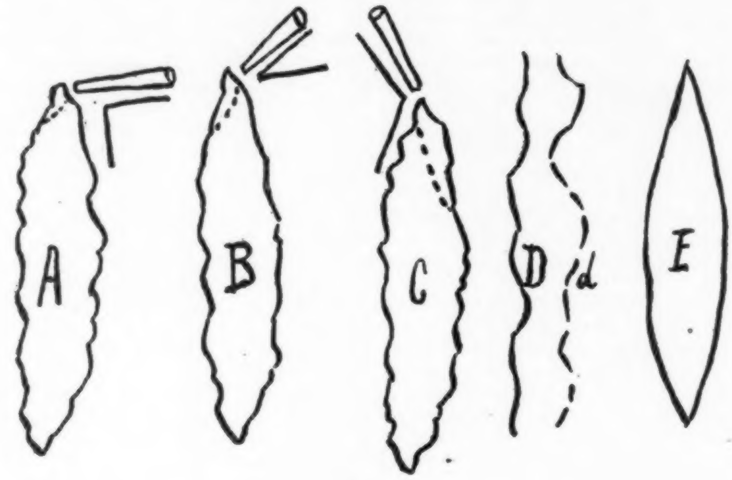


Fig. 1

A flake of any size having been roughed out by beating the flat of the stone around the edge, it was chipped, holding the punch as in A, and the edge was soon straightened as in D, D showing a possible original shape, and further work, using the punch as in B and C soon gave a shape like E, looking lengthwise of the unfinished point. When this result was obtained it did not take long to get a finished point like F. The dotted line X shows the leaf shape obtained from roughing out a probable original shape XX.

To support this theory, Figs. 2 and 3 show an edge and flat view, about natural size, of a flake of gray obsidian ready to be chipped, the dotted lines approximating the original size, and Fig. 4 the finished point. With points in the picture pointing down, the spot of original surface, marked a, b, c, and purposely left, with the deep scar under it will identify this particular piece in the original form and finished. It took half an hour to do the work.

Fig. 5 shows a number of flakes assembled, and Fig. 6 some of them finished. Those on 5 are about one-third natural size, while the finished ones are nearly full size. Unfortunately the same surface of the various pieces does not show in all cases. By notching the bases of 4 and 7, in Fig. 6, knives would be converted into arrow or spear points. Ten was a thick, stubborn triangular flake, and finished would serve as a drill; 1, 2, 3, 4, 7 and 15 are conventional knife patterns, while 12 started to be a fish, but got too thick to be finished. No. 9 is a very common form of scraper or flesher.

Fig. 7 shows a number of drills, 4, 16 and 19; arrow points, 2, 5, 6, 7, 8, 9, 11, 12, 14, 15, 18, 21 and 22. Three was chipped into that shape only because the shape of the flake suggested it. Ten and 17 are knife patterns. Some have been found in California as small as these. A very sore finger is a reminder that anyone attempting to flake a nodule of obsidian would better protect the hand by wrapping the stone in thick cloth, for the flying flakes are often as sharp as a razor blade, and as thin, and will cut to the bone, and there is also danger that the eyes be injured.

I am indebted to Mr. William H. Bertram, of Payette, Idaho, for the material from which these objects were worked. It occurs therein rather small waterworn nodules.

Fig. 3

Fig. 2

Fig. 4

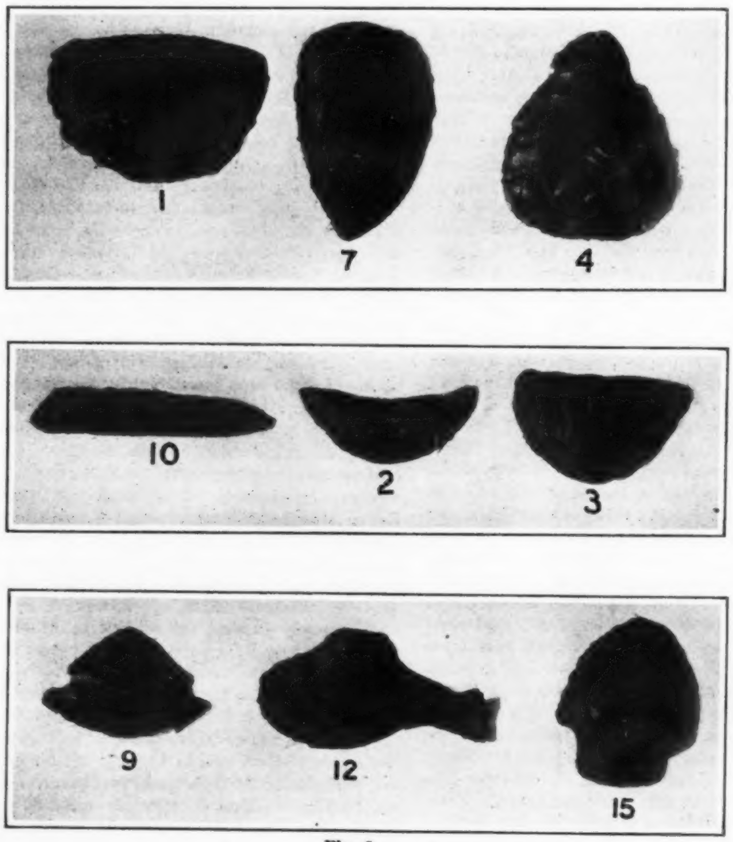


Fig. 6

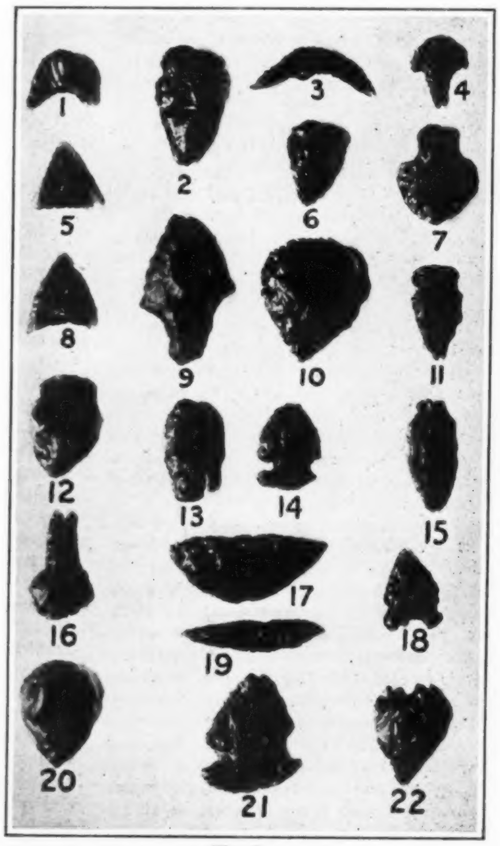


Fig. 7

Nature of Heat as Directly Deducible from the Postulate of Carnot

The germinal idea which developed in the mind of Sadi Carnot in 1824, into the dynamical theory of heat, was that heat can give rise to motive power only in the process of carrying through its effort towards an equilibrium. A proof is now offered that Carnot's principle regarding heat-engines follows from this basic idea by itself alone, without requiring the introduction of any hypothesis as to the physical nature of heat. It then further follows, from applying the same Carnot formula both to direct and to reversed working, that a scale of measurement of heat can be assigned; *i. e.*, an ideal calorimetric substance can be chosen, so that the heat which disappears shall be the equivalent of the motive power that is gained, and conversely; that is, it follows that heat must itself be a form of energy. But a limiting case of this general result requires separate statement from the physical point of view, viz., the ratio of equivalence between heat and work may be so small that practically the heat is conserved as if it were a substance, and then the work may be said to be done by its fall to a lower potential, strictly after the analogy of the fall of water to a lower level. Finally, a second absolute scale of measurement, that of the potential or temperature of heat, may be chosen which reduces the thermodynamic relations to the standard simple form.

It is also remarked that the original Carnot idea involves immediately the complete foundation of chemical physics as applied to isothermal processes; for under isothermal conditions it asserts that the interchanges of heat that occur during physical or chemical transformations do not enter at all into the interchanges of motive power; that is, of isothermal available energy. But physical knowledge was not wide enough for a dozen years after 1824 to enable any general survey of the energies of nature to be thought of, and when the principle of the conservation and interchanges of total available energies come into the light through the theoretical explorations of Faraday, J. R. Mayer, and Helmoholtz, and especially the practical experimental work of Joule, founded mainly on the relations of energy to heat, the Carnot restriction to uniform temperature was tacitly involved, though not overtly expressed until later.

As a chapter in scientific method, it seems desirable to bring into view, even now, the full potentiality that was latent nearly a century ago in the single creative idea of Carnot.—Note from the *Chemical News* on a paper by Sir Joseph Larmor read before The Royal Society.

Hydroelectric Development*

"It is my pleasant duty to appear before you by vote of the Executive Committee of Engineering Council, in response to your kind invitation of January 10th. Being empowered as it is to speak for the American Society of Civil Engineers; the American Institute of Mining Engineers; the American Society of Mechanical Engineers and the American Institute of Electrical Engineers on matters of common concern to all of these bodies, the Engineering Council's official utterances concern only such underlying principles and economic facts as are endorsed by all engineers and beyond the field of controversy. These Societies are scientific and professional. They, therefore, refrain from expressing views on legal, political and commercial questions except when such are closely linked with essential engineering facts. The statements which I am privileged to make to you are not expressions of my personal views nor of those of any group. They have been submitted to, and approved by, the Executive Committee of Engineering Council which believes them to fall within the definition given.

"The introduction of electricity as a means for transmitting power over considerable distances and its subsequent rapid development completely changed the status of hydraulic power. Previously such power could only be used near falling water. Now it is commercially available in convenient form within a radius, in some instances, up to 200 miles, a fact that has made it possible to utilize water powers even when located in remote and inaccessible places. Indeed, today, practically all hydraulic power developments of any magnitude are hydroelectric. Along with improvements in the art of electrical transmission have come equally rapid developments in the application of electricity. Electric light has become almost the universal illuminant. Electric motors largely drive our factories and propel all our street cars. They have made substantial progress in replacing steam locomotives on some large railroads while the manufacture of nitrogenous products for explosives and fertilizers, and of such products as abrasives and aluminum, depends for its commercial success on electrochemistry. In an endeavor to supply the demand for electric current thus created large central generating stations have been established in or near all large centers of population.

"In the light of the foregoing, it might seem reasonable to suppose that a large proportion of the modern demand for electric current would be supplied from the energy in falling water. Such, however, is not the case. Accurate statistics are difficult to obtain but some approximate totals may prove illuminating. It has been estimated by a careful engineer that in 1911 there were over 26,000,000 steam engine horse-power capacity in use (including railroad locomotives), in the United States. The aggregate water horse-power developed and undeveloped has been computed as around 60,000,000. Of this latter the U. S. Census of 1912 gives 4,870,000 as developed and in a report of January, 1916, the Secretary of Agriculture estimates this total to have been increased to 6,500,000. Making liberal allowances for correction in these several figures it seems probable that there are in service from four to five times as many steam as water horse-power and that there are still undeveloped water horse-power equal to at least twice that of all the steam capacity in service. Some of the undeveloped power sites are too remote from any market to be now utilized, and an uncertain number are not commercial prospects; but even so it is clear that the possibilities of additional development are very great.

"There are two fundamental causes which have militated against the substitution of hydroelectric for steam-electric power. One is economic and permanent; the other is statutory and therefore, subject to modification. Both reasons apply to some powers but neither, fortunately, to all. The economic and permanent reason is high cost of development due to natural conditions. Electric power generated by falling water is inferior to that generated by steam in every particular except cost and therefore, water driven service must be cheaper than steam driven in order to justify its existence. The price for service depends primarily on cost and cost divides itself naturally into two main items, namely, operation (including maintenance) and fixed charges. As an hydroelectric plant consumes no fuel its operating cost is less than that of an equivalent steam-driven plant. On the other hand a steam plant costs usually only from one-fifth to one-half as much per unit of capacity as an hydroelectric plant so that the latter must carry very much heavier fixed charges. This disability of water service is usually even greater than the ratio of the costs of two equivalent complete developments. A power enterprise seldom comes into being with a market for its entire ultimate output. Therefore, when steam is to be the motive power, only such capacity is installed as initial demands require and the cost per unit is fairly proportional to that of the ultimate development. In a water development on the contrary, a large part of the cost is for riparian rights, for the dam, impounding reservoir, flume, forebay, etc., and for the transmission right-of-way, towers, etc., which must be at the start largely provided and constructed for the complete installation. The obvious result is a greater fixed charge per unit of capacity and a higher cost per horse-power delivered for sale. In forecasting the commercial prospects of a power enterprise the possible market must be studied and of course, a sale price for power decided upon. As this price is controlled by the cost of similar service from other sources, usually from steam, and as it must be attractive from the start, the additional burden of fixed charges on the initial part of an hydroelectric installation frequently forces the sale of its power below cost. The projectors of the enterprise then must rely for success on a sufficient subsequent increase in their markets. The possibility of an incorrect forecast of the extent of such increase and of the time when it may come imposes a serious business hazard against water and in favor of steam.

"It has been frequently pointed out that as the nation's coal supply is depleted, the cost of coal must rise, thus increasing the cost of steam-electric power as a competitor and raising the market value of hydroelectric power accordingly. The rising price of coal is a matter of record, but it is not so generally known that the improved efficiency of steam-producing machinery (boilers, engines, generators and auxiliaries) has more than kept pace, so that the net cost of producing electric power from coal has steadily declined. As applied to the pre-war period it may be stated that over a period of ten years the cost of coal has risen on an average one per cent per year while the cost of electric power produced from coal has fallen on an average two and one-half per cent per year. In addition to these facts—still referring to pre-war conditions—the cost of steam-electric generating equipment has been greatly reduced. This fact is due partly to the introduction and subsequent improvement of the steam turbine, and in part to the great increase in the size of the units now available. There is nothing to indicate that the limit of improvement in the design of steam prime movers has been reached or is even in sight. It is, therefore, a reasonable assumption that further advances in the art will continue to occur and to cut down both the fixed charges and the operating cost of steam power as a competitor of water. The largest modern steam turbine has now some twelve times the capacity which the largest reciprocating engine had fifteen years ago. Stated another way, the cost of a steam-electric plant per unit of capacity just before the war was about one-third what it was fifteen years previous, while the energy it produces per pound of coal has increased 50 per cent. In addition to the development of steam prime movers the Diesel or the internal combustion engine is now coming largely into use as a further competitor of water power where fuel oil is available as in the southwestern district of the United States. The efficiency of these engines is considerably higher than that of the small size steam turbine and reciprocating engine. There has not been a like improvement in the efficiency nor a comparable reduction in cost of the small reciprocating steam unit and a natural result has been expansion of the central stations. As bearing on the water power situation, obviously many sites which 15 years ago might have been developed to sell energy in successful competition with steam at its then cost could not now be so developed, and in consequence their development is no longer commercially possible. The cost of producing power from either water or steam is a function of load. Fixed charges remain practically unchanged in both instances where the output in energy be large or small but with a steam plant, increased output means increased fuel consumption while a water plant operates either with or without load with but little variation in expense. To illustrate by a concrete example representing not unusual conditions, suppose we assume a steam plant using 2½ pounds of coal per kilowatt-hour at a price of $3 per short ton and having a plant or output factor of 35 per cent—that is to say an output equal to 35 per cent of its theoretical output if every unit were loaded to capacity 24 hours each day of the year. Under these assumptions the cost of fuel per unit of installed capacity per year would be $11.50 and if the other operating and maintenance charge be assumed to fairly offset those of a water installation of equivalent size, $11.50 represents the additional fixed charges which the hydroelectric plant could carry and produce power at an equal cost. If the fixed charges (interest, taxes, insurance and amortization), total 11.5 per cent, therefore, the hydroelectric investment per kilowatt capacity could exceed that of steam by $100. This is not an abnormal excess. Many hydroelectric developments exceed the cost of equivalent steam-driven systems by much greater amounts in which cases they become commercial prospects only if either coal be more expensive per unit of output, or the plant factor be higher, or some other operating or maintenance condition be more favorable. Further, as has been previously stated, hydroelectric power is inferior to steam-electric power. The reasons are elementary. Stream flow is subject to seasonal variation, and therefore to complete or partial interruption by drought in summer and by ice in winter. Floods are a menace. Long transmission lines may break from wind or sleet or the service be disarranged by lightning. The losses on such lines vary with load and are frequently responsible for annoying pressure variations. On account of these and other reasons hydroelectric power cannot prevail against steam competition at the same or a slightly lower price. It must be materially lower.

"We do not mean to imply that water power may not be a commercially practicable competitor of steam. Many successful hydroelectric installations give substantial proof to the contrary. We do wish most emphatically to combat, however, the widely held but mistaken view that any water-driven plant will produce power at lower cost than steam can and that the margin is so large investors generally are eagerly seeking a chance to put money into hydroelectric projects. The most careful investigation, frequently demanding substantial expenditure, and the keenest scrutiny by experts is needed to discriminate between worthy and commercially impractical projects, and the difference is often so small that the imposition of even what seem to be minor burdens is sufficient to turn the scale in favor of steam and entirely prevent what might otherwise be a desirable hydroelectric development.

"The second condition which vitally affects development is statutory. After ten years or more of discussion it has come to be generally agreed that our Federal laws discourage the development of a large proportion of the nation's water powers and remedial legislation has been considered at every session of Congress for many years. The legal obstacles are quite distinct and separate from the economic facts which have been previously described and are in addition thereto.

"Of the estimated 55,000,000 undeveloped water horse-power in the entire country, approximately 40,000,000 is located within the boundaries of the 13 so-called western water power states. In these same states the Federal Government still retains as proprietor 760,000,000 acres, or over two-thirds of the aggregate acreage of all these states taken together. In order to develop power in that section it is, therefore, nearly always necessary to use some part of this public domain if not for the dam site itself, at least for flowage, for transmission right-of-way or for some other purpose. Existing law forbids such use except under permit issued by the Secretary of the Interior and revocable without cause, at any time, by himself or his successor in office. It was once believed that revocation would only follow gross abuse well established by evidence, but the drastic action of a one-time Secretary of the Interior some years since to the contrary, disabused investors of this confidence and demonstrated by a sad object lesson the insecure tenure afforded by existing law. As funds for hydroelectric development must come from private sources, the unstable tenure imposed by this condition has constituted so great a hazard of loss that the private investor has been loath to assume it. The unfortunate—almost disastrous—result has been practical stagnation in water power development for many years. Many available power sites not in the western States, or not on the public domain are on navigable streams. For each such project a special Act of Congress is necessary. The difficulty of obtaining suitable rights by this means has been found so very great as largely to discourage even if not entirely to prevent the developments affected.

Remedial laws recently considered by Congress recognize the essential facts and agree that the remedy is a new law containing the following provisions, namely: An indeterminate permit irrevocable during 50 years except for cause judicially determined, and continuing thereafter unless and until the Federal Government either renews its permit on mutually agreeable terms, or for itself or through a new permittee takes over at its fair value the hydraulic works and certain other parts of the development. The various proposed laws differ as to what parts of the development may be taken; as to whether or not rentals shall be paid and their basis, and in many other particulars. Engineering Council does not consider itself expert in legal matters and will not undertake to discuss the relative merits of the different plans. It should be pointed out, however, that an hydroelectric enterprise being once successfully established, it is alike

*This statement entitled "Hydroelectric Development" was, on the special invitation of the Water Power Committee of the U. S. Chamber of Commerce, prepared by the Executive Committee of the Engineering Council for presentation by its representative, Mr. Calvert Townley, Fellow of the A. I. E. E. and member of the Council, before a committee of the U. S. Chamber of Commerce in Washington on January 14, 1918. Republished from the Proceedings of the American Institute of Electrical Engineers.

to the interest of the owners, of the Government and of the public that it should continue indefinitely without interruption. There is no economic reason to be served by a cessation and the only reasons for providing a legal means of recapturing the installation and the water rights are, first, to preserve an additional measure of Government control against possible abuse by the permittee, and, second, against a remote contingency which might make it desirable that the Government would want to use the power for some other purpose. A successful power enterprise at the end of 50 years will have multiplied the capacity of its initial installation many times, variously estimated at from five to twenty. In doing so, it is almost certain that not only will the entire power available at the original site be fully developed but other powers as well, which latter may or may not be dependent upon Government permits. Still further, in nearly all cases steam plants as well as water plants are necessary. These steam plants are necessary to supplement hydroelectric power at periods of low water and in case of interruption, as well as, in some instances, to provide increased capacity. In fact, modern practise is rapidly approaching that of providing steam capacity equal to 100 per cent of hydroelectric for the purposes stated. In any event, the growth of the enterprise over a term of years will be continuous and progressive. There will never come a time when it may be said to have been completed and subject to no further expansion. This continuing growth makes burdensome and usually abortive any attempt to amortize the investment, while the investment in other water powers or in steam plants or both, interconnected with, and generally dependent for their economic operation on the original development, renders the right to recapture that development only very onerous and one which constitutes a serious impediment to the free and full development of an enterprise which is otherwise most desirable from all standpoints. With respect to power sites on the public domain and on navigable streams the Government is in the position of seeking to have its resources developed without assuming any business hazard and without contributing either capital or credit. It would be unfortunate, in the light of past experience, if any new laws which may be enacted should put the Government in the position of bargaining with capital and of offering just sufficient incentive not to induce capital to undertake the developments desired, thereby, while apparently providing a remedy, in reality insuring a continuance of the present undesirable condition. Hydroelectric enterprises must compete with the demands of other industries for capital. Experience has shown that even without the imposition of additional financial burdens many of them are not sufficiently attractive to secure development and as the attractive prospects grade by imperceptible degrees into the unattractive ones, it is perhaps, self-evident that every additional burden, however small, transfers a percentage of such projects from commercial into uncommercial prospects. It is our belief that the benefits afforded the communities served by cheap power, and to the nation by the conservation of coal resulting from the substitution of a self-renewing for a non-renewable natural resource are far more valuable than is the exact solution of the question of restricting the returns to capital to their irreducible minimum. The present emergency due to the progress of the war has forcibly illustrated the importance of having developed the greatest possible number of water powers as a source of industrial power supply. As it consumes no fuel, the substitution of water for steam power would release to other uses all the extensive railroad and water facilities now engaged in transporting coal. It would similarly release a corresponding volume of labor now occupied in mining this coal and in operating such transportation agencies as well and the boiler-room forces of the steam-power plants themselves."

A Permanganate Electric Cell*

By Arthur W. Warrington

SINCE permanganic acid is a more powerful oxidizing agent than chromic acid, it seems that a cell with the former as depolarizer should be more active than one with the latter. Also there seems to be no adequate reason for taking strong solutions; on the contrary, everything appears against such a proceeding. Accepting the ionization theory, the ions will be relatively more plentiful in a dilute solution than in a strong one.

The Two-Fluid Permanganate Cell.—A carbon plate was separated from a zinc plate by being placed in a porous jar. The capacity of the porous jar was about 250 cc., and that of the zinc compartment about 700 cc. 3.16 grms. of potassium permanganate and, probably, a little over 6 cc. of strong sulphuric acid diluted with water to 250 cc. were placed in the porous jar, and a strong solution of zinc sulphate in the outer compartment. The voltage of such a cell, measured with the only means at disposal, was about 2 volts.

Chemical News.

Two such cells connected in series with a water voltameter and a tangent galvanometer were used in constant action for three hours and forty-six minutes, and the fall of current after the first ten minutes was from 0.09 ampere to 0.085. The experiment was divided into three periods, as the voltameter had to be refilled. During the first period 0.00361 grm. of hydrogen was evolved in 3855 secs., giving a current of 0.0859; during the second period 0.00368 grm. of hydrogen was evolved in 3975 secs., giving a current of 0.0886; during the third period 0.00368 grm. hydrogen was evolved in 4100 secs., giving a current of 0.0859 ampere. These figures are based on calculations made after the first 6 cc. had been evolved. As the platinum electrode was already gray with occluded hydrogen from previous experiments this precaution appeared sufficient. The current was also checked by readings of the tangent galvanometer, which had a constant of 0.150.

The readings of the galvanometer during the second and third periods were very constant. The highest reading in the second period was 30.65° and the lowest 30.25°. In the third period the highest was 29.9° and the lowest 29.4°.

It appears that a little over 6 cc. of strong acid should be added, though the writer's experience is that good results can be obtained with 6 cc. Two cells were made up, each containing, in the carbon compartment, 3.16 grms. potassium permanganate and 6 cc. strong acid, and 14.55 grms. crystallized zinc sulphate in the zinc compartment. The capacity of the carbon compartment was, as already stated, 250 cc., and that of the zinc compartment was about 700 cc. In series with a water voltameter and the tangent galvanometer they yielded the following results:

Time in secs.	Unreduced volume of H.	Pressure	Temp.	Reduced volume of H.	Weight of H. ×10−6.*
398	5	720.2	12.4	4.53	407
818	10	722.9	—	9.10	816
1267	15	724.5	—	13.68	1227
1727	20	727.8	—	18.32	1640
2192	25	730.1	—	22.97	2061

Omitting the first 398 sec., and taking the weight of hydrogen evolved during the other intervals we get:

Time in secs.	Weight of H. ×10 −6	Current	Tangent of galvanometer readings	Current by galvanometer
420	409	0.0934	0.610	0.0915
449	411	0.0877	0.595	0.0892
460	413	0.0859	0.585	0.0878
465	421	0.0865	0.577	0.0866

The voltage at the end of the experiment was about 3.7.

A bichromate cell with 5.88 grms. potassium bichromate and 13.72 grms. strong sulphuric acid in the carbon compartment, and strong zinc sulphate in the zinc compartment, was not nearly so efficient. Two such cells, connected in series with a water voltameter and a galvanometer, took 4965 secs. in liberating 0.00327 grm. of hydrogen, the average current being 0.0632 ampere. The galvanometer read 25.1° after the cells had been working for about ten minutes, and only 22° at the end of the experiment.

Single-Fluid Permanganate Cell.—The porous jar was removed from the cell used before, and the zinc well amalgamated. The capacity of the cell was now about 1000 cc. It was filled with an aqueous solution containing 3.16 grms. of potassium permanganate and 6 cc. of strong sulphuric acid. Two such cells connected in series with the water voltameter and the tangent galvanometer yielded 0.00415 grm. of hydrogen in 8780 secs. The current rapidly fell away. At the end of the first ten minutes it was 0.0538 ampere, and at the end of the experiment it was only 0.0358. The voltage of the two cells at the end of the experiment was about 3 volts.

The yield of electric current, by a two-fluid permanganate cell, becomes very high when the solutions used are very dilute. In the carbon compartment, of capacity 250 cc., was placed a solution containing 0.79 grm. of potassium permanganate and 1.5 cc. of strong sulphuric acid, containing about 2.64 grms. of acid. In the zinc compartment, of capacity 700 cc., was placed an aqueous solution containing 3.59 grms. of crystallized zinc sulphate. The cell was connected in series with a tangent galvanometer and a copper voltameter. The results are tabulated below.

Time in min.	Tangent of galvanometer readings	Time in min.	Tangent of galvanometer readings
0	1.61	100	0.763
10	1.257	110	0.733
20	1.199	120	0.706
30	1.118	130	0.681
40	1.043	140	0.663
50	0.985	150	0.640
60	0.931	160	0.622
70	0.884	170	0.608
80	C.842	180	0.588
90	0.803		

Assuming that the constant of the galvanometer was K = 0.150, and applying Simpson's rule, the quantity of electricity yielded was

$$Q = \tfrac{1}{3} \times 10 \times 60 \times 46.572 \times 0.150$$
$$= 1397 \text{ coulombs.}$$

The weight of copper which should have been deposited was

$$W = 0.00034945 \times 1397$$
$$= 0.46 \text{ grm.}$$

The actual amount of copper deposited was 0.48. The difference between the calculated and the found was due to the uncertainty of the exact value of the tangent galvanometer constant and the poorness of the balance. The galvanometer itself was a reasonably good one. The writer is waiting the arrival of better instruments, delayed by local disturbances, before making more than preliminary experiments.

Now this 0.48 grm. of copper must have required the presence of $(0.48 \times 98)/63.6 = 0.74$ grm. of sulphuric acid. The 1.5 cc. of acid actually used probably did not contain more than 2.64 grms. of pure acid. Hence, with a considerable quantity of energy still available, 28 per cent had already been yielded.

Examination of Cocoa Powders for the Content of Husks

ALL cocoas examined by the author during the last six months have been adulterated with husks, and a simple, trustworthy method of estimating the latter is very desirable. The author criticizes various chemical and mechanical methods which have been proposed and describes in some detail a method based on his observations that the ethereal extract of pure cacao kernels is colorless or faintly yellow whilst the extract of the husks is distinctly brown (Apoth.-Zeit., 1915, 560). Two grams of the cocoa powder and 15 cc. of ether, are frequently shaken for 24 hours in a well-stoppered vessel at the ordinary temperature, the shaking being discontinued during the last 2 or 3 hours. Ten cc. of the supernatant liquid is pipetted into a test-tube and filtered through kieselguhr, repeatedly if necessary, until a clear filtrate is obtained, into one comparison tube of a colorimeter. The test-tube and filter are washed with so much ether that after the washings have been added to the solution in the comparison tube the liquid has a depth of 5 cm. The second comparison tube contains water having a depth of 5 cm. and to it is added ferric chloride (the liquid must be freshly prepared from officinal *liq. ferri sesquichlor.* and contain 0.1 grm. of iron in 100 cc.) until the colors of the liquids viewed from above have the same strength. The comparison is repeated, with the difference that the quantity of water is diminished by a volume equal to that of the ferric chloride solution added. The volume of ferric chloride solution required increases with the husk content of the powder. Pure cocoa powder containing 54 per cent of fat requires 2.4 cc. of the ferric chloride solution; pure husk-free powder requires 1.4 cc.; pure husks require at least 3.5 cc., the average value being 4.4 to 4.5 cc. The author is of opinion that powders containing 20 per cent or more of fat and requiring 2.5 cc. or more of the ferric chloride solution have been adulterated with husks. —Note in *Jour. Soc. Chem. Ind.* on a paper by O. KELLER in *Arch Pharm.*

Brewing Materials

THE author discusses the use of potato-starch, manioc, chit-malt and raw barley as malt adjuncts. The first is satisfactory (cp. Koudelka, *J. Soc. Chem. Ind.* 1917, 399) but expensive. Manioc was employed in several French breweries about twelve or fifteen years ago, until a very heavy import duty was placed upon it because it was stated to be cyanogenetic. When digested with water it produces about 3 mgrms. of hydrogen cyanide per 100 grms., but this small amount is entirely eliminated in the course of brewing. Manioc requires cooking, like rice or maize, and owing to its slightly alkaline reaction it is advisable to add a small quantity of acid, *e. g.*, 75 to 100 grms. of phosphoric acid per hundredweight, before the cooking process. Treated in this way it forms an excellent malt-adjunct. Experience of French brewers with the short-grown or chit-malt advocated by Windisch (see *Jour. Soc. of Chem. Ind.*, 1917, 1188), has been unsatisfactory. It is difficult to grind properly without a special mill, and does not saccharify completely during the ordinary mashing process; even when it is first cooked to gelatinize the starch, saccharification and wort filtration are unsatisfactory. The author considers that raw barley might just as well be used, as all malting expenses are thus eliminated; the raw grain is ground, preferably in a special mill, and then cooked, or it may simply be steeped as if for malting and cooked without grinding if it is well agitated during cooking. The raw barley may amount to 10 to 12 per cent or more of the malt employed.—Note in *Jour. Soc. of Chem. Ind.* on a paper by P. PETIT in *Brasserie et Malterie.*

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The repairs on this engine are nearly completed, and in a few days it will be placed on its running gear

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A locomotive stripped for thorough inspection and repairs to both boiler and machinery

The Breakdown of Our Railway Transportation

Neglect of Motive Power Maintenance the Key to the Situation

WE who ride in the "varnished cars" are often at a loss to understand the frequently published reports that the railroads are unable to handle the supplies that are not only essential to the existence of the inhabitants of our cities and towns but as well as for our armies both abroad and at home. At every terminal point, and scattered at frequent intervals along the thousands of miles of tracks all over the country, are seen herds of great locomotives, hissing with steam, the embodiment of vast pent up power. The boiler is there, the wheels are all there, nothing seems to be missing from the machinery. Why can't they get the trains over the road?

Unfortunately this is also the view taken for many years by the officials who have been in charge of our railway management. They are wise in the intricacies of the stock market; they provide wonderful and luxurious trains for the transportation of their passengers, but when it comes to the grimy details of the machinery that keeps the trains moving they have shown nost remarkable obtuseness. The passenger trains are the ones that make the show to the public, and no expense is spared on them; but it is the unpicturesque freight train that earns the money, and, strange to say, this is the direction in which petty economies have been carried to the extreme, and as long as a freight engine can turn a wheel it is kept at work, whether it is doing the duty intended, and of which it is capable, or not.

The province of a railroad is to sell transportation, particularly the transportation of freight, and to enable it to perform its vital function effectively the operating machinery should be kept in efficient condition. How this is not done is in a measure indicated by a report made by inspectors of the Interstate Commerce Commission last winter; and one example taken from this report will indicate what inefficiency in this direction means. At a roundhouse of one road there were 22 locomotives outside waiting for repairs. One of these had been waiting for four days; and assuming that this engine was capable of hauling 1,200 tons, when in good order, which is undoubtedly far below its actual capacity, and also assuming that it would cover 65 miles a day with its train, which is also a conservative figure, the delay of four days would mean that the road lost a service that would have moved 312,000 tons one mile; ton-miles being the railroad standard for calculating work done. Stating the case in another way, a mechanical plant costing in the neighborhood of $50,000 was kept idle four days because the owners had not provided the facilities for keeping it in operation. Any business man with manufacturing experience will appreciate what this means. This of course is considering only the unnecessary waiting time, without taking into account the time required for the actual repair work.

Of course this may be said to be an extreme case, but nevertheless it is a picture of what has been going on in railroad practice for years, as can be proven by consulting the files of any publication devoted to railroad matters for as far back as anyone cares to go. Of course there are exceptions to this rule, but they are surprisingly few considering the great number of roads in this country and the enormous amount of business they are required to handle. As a rule, however, the provision made for maintaining the operative machinery would be considered by men on other lines of business who use machinery as preposterously inadequate.

We constantly read glowing statements of the enterprise of the railroads in buying big engines to handle their rapidly growing business, but we hear little of the fact that these same roads have made absolutely no provision for maintaining these great and enormously expensive machines. Take the case of the roundhouse, where locomotives are housed for cleaning and adjustment after a run; very few can be found that will accommodate one of the newer large engines that are now so generally used, and as a consequence the doors cannot be closed to protect either the men or the machine while this necessary work is being done, and in winter weather both are exposed to bitter cold and driving snow and rain. Indeed, in many cases it has been necessary to break out a large portion of the front walls to enable the engine to get in at all. Under these conditions good work cannot be expected. The repair shops proper are equally inadequate both in size and equipment. In a recent issue of the *Railway Age*, the leading publication devoted to railroad matters, the statement was made that "One road owning over 2,000 locomotives estimates that its repair facilities can only be brought up to the proper standard by an expenditure of over $10,000,000. Another road operating about 1,500 locomotives has only repair facilities for 750 and these are old shops with inadequate facilities." The same authority estimates that 60 per cent of the locomotives of the country will have to go through the shops for repairs this summer, if we are to go into the next winter in proper shape; but how this is to be done no one can tell.

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The front end of a large locomotive boiler

The small curved tubes are in the superheater, a device that increases the economy and efficiency of the locomotive. The large pipes at the sides convey the steam to the cylinders.

The lack of shop and housing facilities carries many evils in its train. As there is not room enough in the shops for all the engines needing repairs all but the heaviest work is usually done out of doors, on the open tracks and in winter weather it is evident that all work of this kind must be practically suspended. Moreover, in such track work at least 50 per cent of the workman's time is wasted in going back and forth to the shop for tools, material and the machine work that must be done in the shop, and such work as is done cannot be properly supervised. Still another objection to this practice is that no first-class mechanic will undertake this kind of work, for a competent man can always find employment inside where he will not have to undergo the discomfort and inconvenience of an open railroad yard.

The locomotive repair shop is a standing tribute to the incapacity of those responsible for it. It has already been pointed out that on the great majority of our roads these shops are far too small for the work that ought to be done; but in addition to that, as a rule, the machinery with which they are equipped is ridiculously inadequate, and hopelessly antiquated, with the result that the work done in them is excessively costly. Considered as a whole, the system of locomotive maintenance found on the majority of the railroads in this country could not be more inefficient and wasteful.

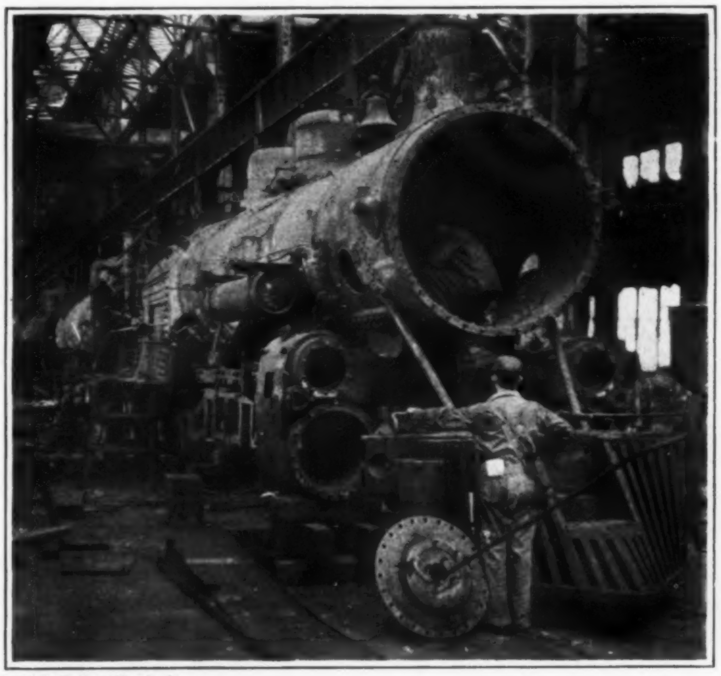
With adequate maintenance equipment for the motive power more work and better work could be done with a smaller number of engines; and the addition of large numbers of new locomotives not only adds to the congestion of the yards, but throws new burdens on the already inadequate shops. Inadequate repair facilities does not mean that heavy losses are incurred only through excessive time required for the repairs, but there are additional losses every day a defective locomotive is kept in service owing to its inability to haul full loads, delays in getting over the road and consequent delays of other trains and general disorganization of the traffic of the road. It can readily be seen that the saving, if it can be called saving, of a few thousands in repair facilities results in the loss of millions of dollars in the legitimate earnings of the railroads of the country, and this has been going on for years.

The failure of our railroads to adequately perform the functions expected of them is, however, not entirely the result of neglected motive power, for similar conditions prevail in most everything connected with freight transportation. The maintanance of freight cars has been slighted to a surprising extent, and terminal facilities have by no means kept pace with the increasing necessities of commerce; although in this matter a considerable portion of the congestion may be attributed to the custom of permitting consignees to delay unloading

cars for extended periods, thus using them for storage purposes instead of promptly releasing them to perform their proper function as transporters of freight. This latter abuse, it may be remarked, has grown up as a result of competition between different roads to secure business by extending privileges to shippers. A typical case of this kind was recently noted in New York, where a number of cars was reconsigned five times, each consignee holding the shipment in the cars until he could resell the goods, the result being that the cars were held out of legitimate service for several months.

That such conditions should exist may seem surprising to most people, who have been regaled with glowing accounts of the phenomenal abilities of the officials who control our roads, and the wonderful growth of the roads they operate. As a matter of fact growth of our roads has been due entirely to the growing demands of the commerce of our country, and in spite of their methods of management. Without question, there have been many men of unusual natural ability in charge of the railways of this country; but results tell their own story, and the serious breakdown of our railroads under the emergency demands of war conditions shows conclusively a great lack of scientific and efficient management, not for the time being, but extending back many years. Indeed, our railroads offer a great field for the efficiency expert.

That some of our railroads have provided modern facilities for caring for their motive power is shown by the accompanying illustrations, which indicate the character of the work that has frequently to be done, and which give an idea of the magnitude of the task.



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Undergoing a thorough overhauling. The size of the various portions of the machine can be appreciated by comparison with the workmen

The Nature and Treatment of Wound Shock

Reference has already been made in these columns to the work of the committee which was established last year on the invitation of the Medical Research Committee to undertake the coordination of inquiries into surgical shock and allied conditions. The first-fruits of the investigations carried out by the committee and by those collaborating with them have recently appeared in the form of three reports issued under the auspices of the Medical Research Committee. It is clear from these reports, which are intensely interesting, that very considerable progress has been made, not only in our knowledge of what may be termed the natural history of shock, but also in its treatment.

Taking, first, the clinical aspect of wound conditions, it has been shown by Capt. E. M. Cowell that wounds may be classified into three groups as regards the incidence of shock. Trivial wounds which do not give rise to shock form the first group. The second group includes moderately severe wounds—*e. g.*, uncomplicated compound fractures of the femur; these patients do not, as a rule, exhibit primary shock, though secondary shock may develop some hours later, if such predisposing factors as pain, anxiety, and exposure to cold are present. In serious wounds, which made up the third group of cases, primary shock appears almost at once, and is most marked in nervous, excitable individuals; partial temporary recovery sometimes takes place, but as a rule the condition of primary shock gradually merges into secondary shock under the influence of cold, pain, and other unfavorable circumstances. In fully developed shock, whether primary or secondary, the systolic blood pressure is always low, but it is clear from the observations of Capt. J. Fraser and Captain Cowell that the height of the blood pressure shortly after the injury depends upon the situation as well as upon the severity of the wound. Readings of the blood pressure taken at short intervals after the patient had been wounded show that the systolic pressure is high (150-170 mm.) in certain cases; high pressures seem to be particularly apt to occur in perforating wounds of solid viscera and in cases of compound fracture of the skull with intact *dura mater*. In such cases, however, the pressure is often very unstable; it is liable to fall suddenly to a low level and the patient then rapidly succumbs.

One of the fundamental features of wound shock is an alteration in the distribution and character of the blood; and these changes, to which earlier workers had called attention, have been very carefully examined by Capt. W. B. Cannon, Captain Fraser, and Capt. A. N. Hooper. They find that the red-cell count and the hæmoglobin percentage of blood taken from the capillaries are higher than of blood taken from a vein. The discrepancy is greater the more profound the shock, and may be as much as two million corpuscles per cubic millimeter. Since the venous red-cell count is approximately normal, the condition is due to stagnation of blood in the capillaries. The condition passes away as the patient improves, and its persistance for several days is an unfavorable sign. Another striking feature of the blood in wound shock is a reduction of its alkali reserve. The alkali reserve consists of bicarbonates, which are the only buffer substances of any importance in blood plasma; and if acid is added to the blood, part of the bicarbonate is converted into a salt of the acid added, carbon dioxide is set free, and the alkali reserve is reduced. Most of the carbonic acid set free is removed by the lungs, but the hydrogen ion concentration of the blood increases slightly, the condition of "acidosis" being thereby produced. Van Slyke has devised an apparatus for measuring the alkali reserve of the blood, and, using this method, Captain Cannon has shown that cases of low blood pressure due to shock or hæmorrhage show acidosis, and that, as a rule, the lower the pressure the more severe is the acidosis. Patients showing acidosis stand operations very badly, and the blood pressure is apt to fall suddenly and to an extreme degree; the reason for this appears to be that acidosis renders the patient much more sensitive to most anæsthetics and to operative procedures.

The low blood pressure, concentration of the capillary blood, and acidosis form, then, a group of symptoms which is present in the large majority of cases of wound shock; and the causation of these symptoms is discussed by Captain Cannon in a review of the various theories which have been put forward as to the nature of shock. The view is now widely held that the essential feature of shock is diminution of the amount of blood in effective circulation, and the question at once arises as to where the blood is held up. The low blood pressure makes it clear that it has not accumulated in the arteries, and, as Captain Cannon points out, the available evidence, both clinical and experimental, goes to show that it is not in the great veins or predominantly in the splanchnic viscera. The experience of surgeons, indeed, in the present war is that in wound shock they have not observed splanchnic congestion on opening the abdomen. Since there is no accumulation of blood in either the arteries or the veins, the blood which is out of effective circulation must be stagnating in the capillaries. There is good reason to believe that the capillary bed is capable of containing a very large proportion of the total blood volume. Further, the capillaries, when distended with blood, tend to lose plasma by filtration into the lymph, and in this way the capillary blood becomes concentrated. Indeed, this is the probable explanation of the concentration of the capillary blood in wound shock. By this means the total blood volume, as well as its effective volume in circulation, may be considerably reduced. When once the process of accumulation of blood in capillary areas has begun, other factors may come in which accentuate the process. Such factors are, first, cold, which in itself tends to produce stasis in capillary areas; secondly, increase in the viscosity of the blood brought about by increase in its concentration; and, thirdly, acidosis, which in itself tends both to dilate the capillaries and to increase the viscosity of the blood. Admitting, then, that, for some still unknown reason, blood accumulates in capillary areas and thereby passes out of active circulation, it is clear that the inflow of blood to the heart, and therefore its output, will diminish; and ultimately the arterial pressure must fall. The causation of the acidosis is not altogether clear, though probably it is partly due to inadequate oxygen supply, first to the tissues in which blood is stagnating and finally, as the output of the heart diminishes, to the body as a whole. Its importance, not only as a factor in aggravating a preexistent stagnation of blood in the capillaries, but also as a predisposing factor in the production of post-operative shock, has been emphasized by Captain Cannon.

The therapeutic measures to which these considerations point are, first, to increase the effective volume of the blood; and, secondly, to remove the acidosis. The rise of arterial pressure produced by injecting normal saline solution is so transitory as to be almost valueless, but Prof. W. M. Bayliss has shown that a prolonged rise of pressure can be produced by injecting a solution containing a colloid, such as gum. The viscosity of such a fluid raises the arterial pressure, and since it does not escape into the lymph its osmotic pressure retains within the vessels the fluid injected. The addition of sodium bicarbonate to such a solution has the effect of also lessening or abolishing the acidosis. Such solutions containing from 2 to 5 per cent gum, suspended sometimes in normal saline, sometimes in hypertonic saline, and sometimes in bicarbonate solution, have now been extensively used in the treatment of wound shock; and the testimony of surgeons is unanimous as to their value, whether they are administered for the relief of shock or as a prophylactic measure at the beginning of an operation. —*The Lancet.*

Gravitational Instability and Figure of the Earth

In an earlier paper [Sci. Abs. 237 (1904)] the author attempted to examine whether, owing to gravitational instability, an arrangement in concentric spherical shells might not be unstable for a planet the size of our earth. It then seemed possible to attack the problem only by a highly artificial assumption, and on that basis it appeared that the symmetrical configuration would be stable under present conditions, but seemed probable that it might have been unstable in past ages when the earth had less rigidity, and that then the stable configuration would be unsymmetrical, the surfaces of equal density being spherical but not concentric, so that the point of maximum density coincided neither with the center of gravity nor with the figure center of the earth's surface. It was suggested that traces of this unsymmetrical configuration might still be found in the arrangement of oceans and plateaus on the earth's surface.

Rayleigh suggested [Sci. Abs. 1191 (1906)] the substitution of the more natural assumptions that we might regard the symmetrical configuration as one in which the gravitational forces were balanced by hydrostatic pressure only, and the additional stress due to disturbance as being connected with the additional strain of the ordinary elastic-solid relation. This definite mathematical problem was attempted by Love in 1911 in his Adams Prize Essay, "Some Problems of Geodynamics," under the limitations that the initial density and the elastic constants were constant throughout instead of being functions of r. He then showed that if the rigidity were rather small, compared with the resistance to compression, the body might be unstable in respect of displacements specified by spherical harmonics of the first degree, although stable as regards radial displacement—a result, he remarks, "distinctly favorable to the hypothesis that the division of the earth's surface into a land-hemisphere and a water-hemisphere might be a survival from a past state in which a symmetrical arrangement would have been unstable." Simple considerations dealt with, however, in the present note seem to show that, apart from highly artificial mathematical possibilities, which could hardly be considered possible in a natural system, when the density and elastic constants are allowed to take their natural course as determined by the configuration of the mass, this result is no longer true.

The spherical symmetrical arrangement appears always to be stable, so that the attempted explanation of the earth's dissymmetry of figure fails entirely.—Note from *Science Abstracts* on a paper by J. H. Jeans in *Roy. Soc. Proc.*

Problems of Atomic Structure—IV*

Differences Characteristic of Different Elements, and Mechanism of the Molecule

By Sir J. J. Thomson

[Continued from Scientific American Supplement No. 2212, Page 327, May 25, 1918]

In beginning his fourth lecture at the Royal Institution, Sir J. J. Thomson, O.M., P.R.S., said that in his previous lectures he had put forward certain views according to which the atom was built up of a number of electrons distributed round a central charge of positive electricity, and certain conclusions had been reached as to the arrangement of these electrons in atoms of various kinds. Before proceeding further it was desirable to try to find some check by which the accuracy of these deductions could be tested, particularly as the subject was difficult in itself and the evidence available was indirect. In all such cases it was important that the consequences of a theory should be continuously subjected to any test that came to hand in order to see how far they were in agreement with experimental results, and further advances should not be attempted till this had been done.

He would, he said, begin with a simple test, which the theory ought to satisfy if well based. Since the theory in question was one concerning the structure of the atom, and not of the molecule, it was reasonable to take as the subject of the proposed test an element such as helium, in which the atmos were also the molecules; and to which, therefore, the theory should be directly applicable. This theory, when so applied, led to certain numerical conclusions which could be compared with the actual experimental figures. The proposed test was accordingly a stringent one.

If the atom were built up in the way suggested in his previous lectures, that of helium would be an arrangement consisting of two positive charges concentrated at the center of the atom, and two electrons symmetrically arranged on opposite sides of this nucleus, as indicated within the dotted circle, Fig. 1. Assuming that the law of force between an electron and a positive charge was represented by the expression given in his last lecture, viz.:

$$\frac{Ee}{r^2} - \frac{h^2}{r^3} \quad . \quad . \quad . \quad . \quad (1)$$

then the position in which the electrons were in equilibrium could be calculated. If this distance were represented by d, then taking also into account the action of the electrons on each other, the value of d was given by the equation

$$\frac{1}{d} = \frac{7}{4} \cdot \frac{e^2}{h^2} \quad . \quad . \quad . \quad . \quad (2)$$

This, then, represented the distance of the electrons in Fig. 1 from the center of the helium atom. Suppose now a large positive charge were brought into the position indicated by A. The effect of this would be to drag down the electrons towards A and to push back the position nucleus. When no external charge acted on the helium atom the center of gravity of the two electrons coincided with that of the positive charges, but when the arrangement was disturbed by an external charge at A this was no longer the case. The balance was disturbed and the atom became an electric analogue of a magnet, and exerted forces which would be manifested as an electrical property of helium, viz., its specific inductive capacity. If we knew the amount by which the centers of gravity of the electrons was displaced with reference to the positive charge, we could calculate the value of the specific inductive capacity of the gas. He had, Sir Joseph continued, made this calculation, taking into account the fact that the relative displacement of the centers of gravity of the positive and negative charges would be different if the external positive charge was situated at B instead of at A. If k denoted the specific inductive capacity, then his result was that

$$k - 1 = Nd^3(8 + 8 + \tfrac{5}{3})\, 4\pi \quad . \quad . \quad . \quad (3)$$

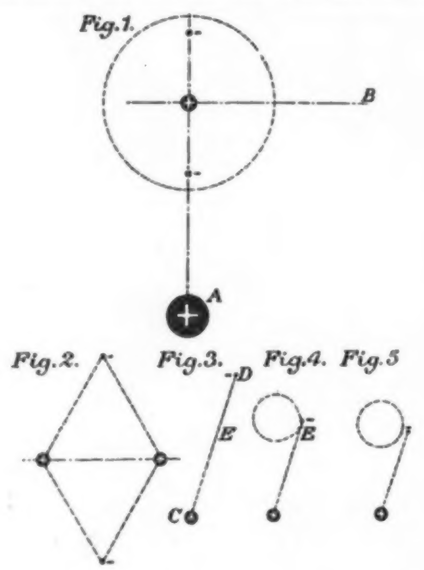
Here N denoted the number of molecules in a cubic centimetre of gas at standard temperature and pressure, while d had the value given by equation (2) *supra*, and might be defined as the radius of the helium atom. Since N was known and the specific inductive capacity could be measured, the value of d could be obtained from (3). It came out as $\frac{1}{2} \times 10^{-8}$cm.

Having thus found d, it was possible to calculate the expenditure of energy required to ionize a helium atom by removing from it an electron. The expenditure of energy necessary for this depended on the central charge, and on the distance d, and on making the calculation it appeared that the ionizing potential of helium should be 24 volts. Actual experiment gave a value of a little over 20 volts, which, considering the uncertainties involved, might be considered as sufficiently in accord with the calculated value as to be confirmatory of the theory on which the computation was based.

A step further could now be made. The hydrogen atom was held to consist of one electron and one central positive charge acting on each other by the law of force given in equation (1). If we knew the value of h in this equation we could calculate for the hydrogen atom, the distance d of the electron from the center of the atom. As had just been shown, d could be determined for the helium atom, and knowing this, h could be found from equation (2). Assuming h to be the same for hydrogen as for helium, we could now find d for hydrogen and could consequently calculate the energy required to remove an electron from the hydrogen atom. Sir Joseph said he had made this calculation, which gave 11 volts as the ionizing potential of hydrogen, which was in very close agreement with the actual experimental value.

So far, therefore, the theory he had put forward had successfully withstood comparison with experiment.

Another test was also possible. So far he had dealt with atoms, but in general the molecules of a gas comprised two or more atoms, and it was desirable to form some idea as to the constitution of these molecules. The simplest case was that of the hydrogen molecule which, if the views he had advanced were well based, must have



some such constitution as was represented in Fig. 2. The two positive charges were situated at opposite corners of a rhombus and the electrons at the other two corners. These electrons served as a cement or mortar to hold the molecule together against the mutual repulsion of the positive charges. If the angles between the sides were right the figure would be one of equilibrium.

A new question now arose. Hitherto we had had to consider only the law of force between a positive charge and an electron, and had assumed that it was more complex than the ordinary law of the inverse square, comprising, in fact, a term representing a repulsion varying with the inverse cube of the distance. On the other hand it had been assumed that the law by which electrons repelled each other was the ordinary inverse square law. In dealing with the configuration of the hydrogen molecule, we had to consider the law of force by which the positive charges repelled each other. Was this the ordinary inverse square law, or was the repulsion on a positive charge numerically equal to the attraction which would be experienced by an electron similarly situated? The angles between the sides of the parallelogram (Fig. 2) would differ in the two cases. If the repulsion of one positive charge on another varied as the inverse square of the distance, calculation showed that the figure of equilibrium was a square, but it also turned out that this equilibrium was an unstable one, equivalent to that of a needle standing on its point. In such case the needle was in equilibrium, as all the forces acting on it were in perfect balance, but an equilibrium of this kind was not of the type for which the engineer or the physicist had the slightest concern. Hence, if the law of repulsion between positive charges was the ordinary one, the hydrogen molecule could not exist, as the two atoms would fly apart.

Taking next the second hypothesis, that the repulsion of one positive charge for another was the same as that which each would exert on an electron similarly situated, the law of repulsion would be:

$$\frac{ee^1}{r^2} - \frac{h^2}{r^5} \quad . \quad . \quad . \quad . \quad (4)$$

From this it appeared that if r were small enough the repulsion between two positive charges was replaced by an attraction. This removed a difficulty which might have been felt in the supposition that the nucleus of an atom was built up of a number of positive units of electricity, there being one such unit in the hydrogen atom, two in helium, and so on. If these positive units repelled each other to the bitter end, how was it they stuck together and did not fly apart under the tremendous forces at work? This difficulty was removed if the law of repulsion was represented by (4), since if the distance between the charges was less than a certain amount, the positive charges would form a coherent nucleus, and a formidable objection to the existence of this central charge was removed.

If the law governing the repulsion of the positive charges was given by (4), the figure of equilibrium was represented by Fig. 2, the two triangles into which the figure was divided by the horizontal line being equilateral, and further calculation showed that in this case the equilibrium was stable.

Having given the configuration of the charges in the hydrogen molecule, it was possible to compute the specific inductive capacity of the gas, and this calculation gave a value which did not differ by 5 per cent from the actual experimental figures. This calculation was based on the assumption that the h in equations (1) and (4) was the same for hydrogen as for helium, and was, in fact, primarily made to test this hypothesis.

Further tests of the theory could be made by studying the kind of light the atom of a gas gave out when it became luminous. There were certain peculiar features about the luminosity of a gas when producing its ordinary spectrum and not merely scattering light derived from some outside source. The most obvious suggestion as to the origin of the light was that the electrons in the atom wriggled about under the shocks received from outside. As they wriggled they gave out electric waves. Were this the case, however, there was no reason why the luminosity should not increase gradually and uniformly as the intensity of the shocks disturbing the electrons was continuously increased. Actually, however, a gas became luminous quite suddenly. For example, if an electric discharge were passed through mercury vapor, beginning with slow cathode rays, no trace of the mercury lines was at first to be seen in the spectroscope. If the speed of the rays was gradually increased, however, a critical value was reached at which the mercury lines suddenly shone up quite brightly, no trace of them being apparent up to this stage.

Many views had been urged as to what was really necessary for a gas to become luminous. Some maintained that an essential factor was the expulsion of electrons from the atom, and the return of these electrons was supposed to be responsible for the emission of the light. If this were true, the existence of free electrons would always be a concomitant of the emission of light, and the luminous gas would be accordingly a conductor of electricity. In general it was the case that gases had the power of conducting electricity when they were giving out light. To illustrate this the lecturer made use of a very small flame of ethyl chloride. This flame was, he said, so cold that the finger could be passed through it, yet when placed between two plates, one of which was connected to earth, and the other to an electroscope, the latter was rapidly discharged.

There was no doubt but that the removal of electrons from a gas was a very powerful means of giving use to radiation, but it was nevertheless not true that in all cases there must be a liberation of electrons for the production of luminosity. Ionization was, in short, not invariably produced when a gas was radiating out its characteristic light.

Cases of fluorescence constituted one exception to the rule. Thus, if a beam of light were passed through a bulb containing iodine vapor at a very low pressure, this vapor gave out light of a distinct and definite color; yet experiment showed that there was no trace of ionization. With other vapors under similar conditions ionization could be detected, but the case of iodine proved

*Reported in *Engineering*.

very definitely that this ionization was not absolutely essential to luminosity.

He proposed, therefore, to consider how there might be the possibility of big disturbances of the electrons in an atom, without the production of ionization. In his last lecture he had assumed for reasons of brevity and clearness of exposition, that an atom contained, in addition to positive and negative charges, certain hypothetical units of repulsion or Q? particles. Suppose that from an atom in a state of equilibrium one of these Q? particles was expelled by some process. As a result, an electron would not be repelled as much as before, and would take up a new position of equilibrium. In moving from its original position to its new one a finite and definite amount of energy would be liberated. If this energy appeared as radiation, this radiation would be produced without the liberation of an electron from the atom, and there would consequently be no ionization.

If the law of force were represented by equation (1) *ante*, then the energy liberated when an electron moved from a position where h had the value h_1 to a position in which h was equal to h_2, was

$$A\, e^4 \left(\frac{1}{h_1^2} - \frac{1}{h_2^2}\right),$$

where A was a constant which he would not define further for the present.

Suppose that the expulsion of one Q particle caused a unit change in the value of h and, further, that h_1 corresponded to the presence of a number p of these units of repulsion, and h_2 to the presence of q such particles. Then in changing from p to q the energy liberated would be

$$A\, e^4 \left(\frac{1}{p^2} - \frac{1}{q^2}\right)$$

where q and p were both integers. This energy was accordingly represented by the difference of the inverse squares of certain integers.

Planck had enunciated a law that when radiation was produced from an atom the frequency bore a constant ratio to the energy change.

Hence, if the energy change obeyed Planck's law the frequency should be expressed by a formula, corresponding to (5), which was equivalent to Balmer's law, and gave a simple explanation of it. It would be seen that with quite a small number of Q particles all kinds of spectral lines might be produced. If the electron were far out there would be a small change of energy, as it moved from one position of equilibrium to another, but if it were near the nucleus the energy change would be large and the frequency (by Planck's law) of the light emitted would be very high. All kinds of lines could thus be produced. The number of possible changes of energy being proportional to the square of the number of the hypothetical Q particles.

One notable peculiarity of spectra was the extraordinary variations produced in them by changes in the physical conditions of the experiment. This the speaker illustrated by passing the discharge of an induction coil through a vacuum tube filled with argon. When the induction coil was coupled up directly with the tube the light emitted was pink in color, but on including two leyden jars in the circuit, the light changed to a brilliant blue. When the gas in the tube was nitrogen, Sir Joseph pointed out that the color of the light emitted was different in different parts of the tube. In another experiment a bulb filled with mercury vapor was wound with a coil of wire, through which was passed the oscillatory discharge from a set of leyden jars. The color of the light emitted by the mercury vapor under this stimulus varied with its distance from the center of the bulb. Near the exterior it was pinkish, while further in it had a green hue. When photographs were taken of the spectra of different gases submitted to an electrodeless discharge of this kind, the difference in the character of the light from the inner and from the outer layers was, the speaker said, very marked. Some lines appeared in both spectra, while others showed up in one only.

Recurring again to the mechanism of radiation, the lecturer said that if an electron were entirely ejected from an atom, radiation would be produced on its return, and in that case luminosity was accompanied by ionization. We might also have, however, a falling in of electrons already inside the atom owing to the displacement of a Q particle. In this case the gas would not become a conductor.

It was of interest, he went on, to consider in a grossly mechanical way how we might picture the process.

Let C, Fig. 3, represent the positive nucleus of the atom, and D an electron. Between them would extend a tube of force represented by the dotted line. Suppose now that the electron at D fell down into a new position E, dragging behind it the tube of force as represented in Fig. 4. On reaching E this tube of force would join up with C E, leaving a complete loop as represented in Fig. 5. This loop would then rush off through space as a unit of radiation, carrying with it an amount of energy proportional to the fall from D to E. We could imagine in this way that a radiating atom shot off these systems of loops or closed tubes of force, each carrying with it a definite amount of energy. How these emissions give rise to definite frequencies was another matter, which he proposed to discuss in his next lecture.

[TO BE CONTINUED]

Properties of Portland Cement Having a High Magnesia Content

PORTLAND cement with a magnesia content of about 9.50 per cent may be burned in a rotary kiln without producing a clinker materially different from one containing less than 4 per cent. The clinkering temperature will be reduced somewhat, however. With greater amounts of magnesia present the resulting clinker is very vitreous and dusts more or less slowly, the rapidity and amount of dusting increasing with the magnesia content. High-magnesia clinker is of a light-brown color, in strong contrast to the usual dark, glistening, normal clinker. The resulting ground cement is of a light-brown color, which makes a concrete decidedly different in color from concrete made from normal cement.

No new constituents are present in the high magnesia cements until the magnesia has reached about 8 per cent. A very small amount of the mineral monticellite ($CaO.MgO.SiO_2$) is then noted. The amount of this constituent increases rather rapidly with the increase of the amount of magnesia. Further, when the latter has reached about 10 per cent, the mineral spinel ($MgO.Al_2O_3$) is noted; this also increases in amount as the magnesia increases. There was about 23 per cent of monticellite present in the clinker made from dolomite only (25.5 per cent MgO). The amount of spinel was not determined in this case, but it was not more than a minor constituent.

With the largely increased amounts of magnesia the amount of orthosilicate of lime was very decidedly decreased. The amount of tricalcium silicate was not affected materially. As the amount of spinel was very small in any case, the amounts of tricalcium aluminate were only slightly decreased in the case of the higher magnesia clinker.

The structure of the very high-magnesia clinkers was decidedly different from those of normal composition. The former were much more crystalline in character, the crystals having developed to a comparatively large size and with well-defined outline. In normal clinker the crystals are not only small but usually lack outline, being fused into and surrounded by the invariably poorly crystallized orthosilicate.

When the magnesia content did not exceed 9.5 per cent, there was no marked change noted in the time of set, though there was apparently a slight tendency toward slower final set. When the magnesia exceeded this amount, the initial set became quite rapid, accompanied by a marked evolution of heat. In some cases this produced a drying out, which caused the appearance of a quick, false, final set. In other cases the evolution of heat was not so marked, and there resulted a slow final set. This quick initial set could be predicted from the changes noted, by the microscopic examination, in the structure and the character of the constituents of the clinker. In this class of cements the constituents are well defined and not surrounded so uniformly by the slow-hardening orthosilicate. Furthermore, the amount of this latter constituent is materially reduced in the high-magnesia cements, while the quickly setting aluminate and tricalcium silicate are but slightly changed in amount.

With the exception of two of the very high-magnesia cements the other high magnesia cement pats showed these cements were sound either when stored in water for 28 days or in air for 28 days or subjected to boiling water or steam at the end of 24 hours. These two unsound cements developed a very slow final set, and when subjected to the boiling and steaming tests had not attained their final set; consequently, after the test they were soft and somewhat enlarged.

The strengths developed, either by the neat cement or 1:3 sand mortar or 1:1½:4½ gravel concrete, show that cements containing as much as 7.5 per cent of magnesia are satisfactory. It would be impossible to predict from the strength tests at the end of one and one-half years which were the cements containing low magnesia or magnesia up to 7.5 per cent. With higher amounts the strengths developed decreased with increased magnesia, but even with the high-magnesia cements there is a notable increase of strength with age.

The strength of the concretes subjected to the action of the solution of salts was not materially different from that of those subjected to the action of water. While some of the specimens did show a slight disintegration, it was not sufficient to affect their strength at the last period at which they were tested, and, furthermore, it appeared in the case of concrete made from cements of moderate magnesia content and was not noted in those of high magnesia content.

The high-magnesia cements contain a large amount of "insoluble residue." This residue is the monticellite and spinel, both of which are very largely insoluble in dilute acids.

Measurements to determine changes in the volume of the concrete with the progress of hydration, show in some cases a slightly greater change produced by the high-magnesia cements than by the low-magnesia ones.

Strength specimens to be tested at still later periods than those reported in this paper are on hand. It may be that at very late periods some of the conclusions which appear justified from the present results may require revision.

Attention should again be called to the fact that the work presented in this paper was not undertaken to show the advisability of allowing a greater magnesia content in cement, but only to determine how greater amounts of magnesia affect the constitution and properties of cement of normal compositions.—Summary of Technological Paper No. 102 of the Bureau of Standards, by P. H. BATES, Chemist of the Bureau.

Engineering Feats of the Ancients

IN an address at the annual meeting of the American Society of Civil Engineers the President of the Society, Mr. George H. Pegram, made some statements in regard to the antiquity of the engineering profession, and to the achievements of days long past, which, while not new in detail, are of interest, and most impressive, as presented by the speaker. He said:

"Engineering is sometimes claimed to be a modern profession because the present appellation is of comparatively recent adoption. The practice, however, is of such ancient origin that, in considering its relations to the community, we are obliged to view its history. Most of our tools—the dowel, drill, chisel, wedge, screw, pulley, file, and saw—were used by the ancients.

No works of modern times compare in magnitude with those of the ancients. Consider a reservoir, to impound the waters of the Nile, covering an area of 150 square miles, with a dam 30 feet high and 13 miles long. The pyramids of Gizeh, constructed more than 5,000 years ago, had granite blocks which were 5 feet square and 30 feet long, and were transported 500 miles. One of the temples of Memphis was built of stones which were 13 feet square and 65 feet long, and laid with close joints. The Appian Way from Rome to Capua was so well built that after a thousand years its roadway was in perfect condition, and, even now, after two thousand years, with slight repairs, is in use. The modern engineer would question the possibility of such work, without these great examples.

If one could imagine cessation of life on this continent, and our works subjected to the destructive forces of time and Nature for a thousand years, what evidences of civilization would remain?

Probably the most surprising and interesting of the older examples of engineering are the inventions of Leonardo da Vinci, as shown in his sketches. He seems to have lacked nothing but the application of mechanical power to produce most of the typical machines of the present day. The bellows-blast, jig-saw, lathe, rolling mill, printing press, file-cutting machine, trip-hammer, sprocket-chain, water-wheel, boring machine, rapid-fire gun, spinning machine, side-wheel boat, flying machine, etc., and I commend to your notice the work of Franz M. Feldhaus on "Leonardo da Vinci as Engineer and Inventor," published in 1913. In this work is shown an apparatus of cylinder, piston, and valve by which Leonardo determined the relative volumes of steam and water; also an atmospheric engine, consisting of cylinder, piston, and valve, by which reciprocal motion was produced.

The pity is that, in all his machinery requiring mechanical power, and in spite of his experiments hinting at its application, he still was limited to muscular effort, and it was not until the invention of Watt that the mystic wand of mechanical power initiated a transformation of the world and made a radical change in the organization of man.

We look in vain for the application of mechanical power by the ancients, whose works seem almost impossible without its assumption, but the stone reliefs showing the movement of large weights by manual power indicate that probably the other did not exist.

The Properties of Oils*

And Their Relation to Lubrication

George B. Upton[1]

THE ordinary users of oils for lubrication of machinery and many of the sellers, have very little notion of what those properties of oils are which are concerned in lubrication, and how lubrication comes about. Hence the following brief sketch of the meanings and purposes of various tests to which oils may be put.

1. *Test for Acidity.*—Acids in oils, in amounts greater than traces, cause rusting and pitting of metal surfaces, and will soon put shafts or bearings out of commission. Alkalis must also be avoided. Oils should be chemically neutral, neither acid nor alkaline. All oils form acids more or less rapidly in the presence of heat and moisture; more rapidly the greater the heat. With mineral oils this action is usually negligible; with animal and vegetable oils it is considerable. Soaps used to alter the viscosity of oils also decompose by heat. Mineral oils are often bleached during manufacture by the use of sulfuric acid, which may fail of removal or neutralization in the later processes, and so remain in the oil as sold. An oil which is neutral when purchased, and which does not get very hot in use, can be expected to stay neutral.

2. *Test for Ash.*—All pure oils, when heated and ignited in the open air, burn quite completely, leaving almost no ash. Mineral soaps put into the oils leave ash. The ash test, therefore, becomes practically a test for the presence of soaps in the oil. Whether or not these soaps are objectionable depends upon the use to which the oil is put. Soaps usually raise the viscosity of oils. Soaps are used freely in greases, and generally without objection. If the oil is to be used hot, however, the soap is liable to decompose and cause trouble by rusting of metals.

3. *Test for Carbon Residue.*—The oils consist principally or entirely of chemical compounds of carbon and hydrogen (hydrocarbons). About 90 per cent by weight of the oil is carbon in the ultimate analysis. When heated very hot the hydrocarbons decompose partially, forming volatile combustible gases and heavier hydrocarbons, or carbonaceous matter, and even elementary free carbon. Such free carbon (and carbonaceous matter), is always in solution or suspension in oils to some extent, from the processes of manufacture. Heated very hot in closed vessels, without ignition, the oils partially distill unchanged, and partially decompose. When this action is carried to the end point there is left in the flask a thin deposit of "carbon residue." Animal and vegetable oils, and mineral oils loaded with rosins or soaps, leave larger "carbon residues" than pure mineral oils, for which the residue is only a fraction of one per cent.

The "carbon residue" test has practically nothing to do with the amount of "carbonizing" by the oil in the cylinders and on the pistons of internal combustion engines, though it is often so taken. The conditions of the test are utterly different from the conditions in the engine cylinder. In the cylinder the oil is spread out in thin films on hot metal, and exposed to the burning gases of the "explosion." These gases may or may not carry an excess of oxygen to combine with the heated oil. The amount of carbon formed in the cylinder depends on the amount of oil reaching the cylinder and the conditions of exposure in the cylinder. The amount of oil depends on the mechanical fit of piston and cylinder, especially the condition of the piston rings, and on the viscosity of the hot oil on the cylinder walls. The lower the viscosity the more oil will go up past the pistons. After the oil has gone past the pistons a "lean" mixture in the combustion space will have excess oxygen to burn up the oil more or less completely, while a "rich" mixture will not have excess oxygen, the oil cannot burn, and the rich mixture may itself be depositing lampblack carbon.

4. *Oxidation Test.*—Oils gradually oxidize by contact with air at exposed surfaces of oil. The rate of oxidation increases with temperature. The product of the oxidation is resinous or gummy, and spoils the oil as a lubricant if much oxidation occurs. Mineral oils oxidize very little or not at all. Some, but not all, of the animal and vegetable oils oxidize very rapidly. An extreme case is that of linseed oil. The action commonly called "drying" of paint is really the oxidation of the linseed oil. A lubricating oil should be practically non-oxidizing.

An oxidizing oil brings in a considerable fire risk, because when spread out thin on oily waste or cloths it will often oxidize so rapidly at ordinary temperatures as to start "spontaneous combustion." This does not happen with a non-oxidizing oil. Any oily waste will burn strongly after ignition; but a lubricating oil ought not to be able to start a fire of its own accord.

5. *Volatility Test.*—Oils vaporize without chemical change, just as water does. In some cases, where the oil is kept quite hot, the loss of oil by this vaporization, or volatility, may be perceptible. Usually the vaporization loss is small, and even less than leakage losses from a machine. Mineral oils are generally more volatile than animal or vegetable oils. Volatility of lubricating oils is of little practical importance.

6. *Surface Tension, or Capillarity.*—This, and the adhesion of oil to metal surfaces, are the forces which cause oil to spread over the metal, and to crawl into the thin spaces between bearing and shaft, or piston and cylinder, while the metal parts are at rest. Animal and vegetable oils adhere better to metal surfaces than do mineral oils. This property is especially important in steam engine valve and cylinder lubrication, where the oil must adhere to the metal so strongly that water, and the tendency of oil to float on water, cannot remove the oil film. Mineral oils, pure, may be displaced from a steam engine cylinder wall or valve seat by hot water. The adding of small amounts of animal or vegetable oil to the mineral steam cylinder oil helps the oil film to adhere. The good effect is important enough to counterbalance the bad tendency of the animal or vegetable oil to form acids. Where water is absent the mineral oils stick well enough on metal surfaces. Surface tension of lubricating oils is not usually noticed in testing.

7. *Color Test.*—Color of oil has no connection with lubrication. It results from the details of the manufacturing process. Mineral oils are naturally dark, or dark red, until the carbon and carbonaceous matter are removed by filtration or bleached by sulfuric acid. The removal or bleaching of the carbon and carbonaceous matter probably does little to affect the lubricating properties, though it may make the oil prettier to look at.

8. *Emulsion Test.*—A very simple test to check the purity of a mineral oil is the emulsion test. Fill a clean bottle about a quarter full of clear water (preferably distilled), and another quarter full of the oil. Shake vigorously until oil and water form an emulsion. Allow to stand for 24 hours. A pure distilled mineral oil separates completely from the water, which remains clear. Acids, soaps, rosins, etc., make emulsification easier, prevent the complete separation of oil and water, and leave the water cloudy. There will often be, after the separation, a layer of leathery foam between the oil and the water. The emulsion test is a rough one; it takes a chemical analysis to work out the real reasons for the actions shown. The fact that an oil will or will not readily form an emulsion with water is often of importance in judging whether the oil will feed or not, as an oil pump suction will not work on the foam.

9. *Flash Point, and Burning Point or Fire Test.*—It has been mentioned that when heated to high temperatures oils decompose chemically, the hydrocarbons breaking up into volatile combustible gases and carbonaceous matter, or even free carbon. The flash point is the lowest temperature at which the decomposition of the oil by heat becomes demonstrable through the formation of a combustible, slightly explosive gas mixture over the oil surface. The burning point, or fire test, is the lowest temperature at which the production of combustible gas from the oil is so rapid as to maintain combustion after ignition. The oil does not ignite itself merely because it is at or slightly above the flash point or the burn point; external ignition is necessary.

These tests originally came in because of fire risk. The flash and burn points of lubricating oils were a generation ago often as low as 200° to 300° F. An ordinary hot bearing was potential material for a bad fire, for an oil fire is hard to stop after it gets under way. With improvements in the manufacture of oils, the flash and burn points have been raised so that they now lie usually above 300° F., and often above 400° F.

As regards fire risk, the flash point of lubricating oils is of little consequence; the burn point is the more important of the two. As regards the use of the oil as a lubricant, the flash point is more important than the burn point. The flash point puts an upper limit to the temperature of use of the oil, because the chemical decomposition changes the properties of the oil, and the formation of gas in an oil film will break the film. It is quite possible, even probable, however, that the practical upper temperature limit for the use of an oil as a lubricant occurs below the flash point, from the decrease of viscosity of the oil with rising temperature. We test for flash and burn points, therefore, to catch any oil which might bring fire risk from having these points too low, and to be sure that the flash point occurs at a temperature higher than the highest temperature at which the oil is to act as a lubricant.

There are a number of places in machinery where oils have to lubricate at decidedly high temperatures. Steam engine cylinder oils must lubricate valves and pistons at the temperature of high pressure steam, somewhere from 300° to 400° F.; with superheated steam even higher. Automobile cylinder oils must lubricate the pistons in the cylinders with a temperature in the oil film of 250° to 350° F. In this case a further requirement is put upon the oil that that portion of it which goes up past the pistons must burn up quickly and cleanly. Hence, the flash and burn points of automobile engine oils must be neither too low nor too high, say not under 400° nor over 500° F. for water-cooled engines. Stationary gas engines and marine engines run colder than automobile engines, and the flash and burn points of their oils may be lower.

10. *Heat Test.*—While the oil is undergoing decomposition by heat we are interested not only in the gases given off, which give the flash and burn tests, but also in the solid or semi-solid products of the decomposition. These substances, carbon or carbonaceous matter, remain in the oil. At present there is no standardized test on this line, paralleling the flash and burn test. It may be suggested that after the flash point temperature is known, another sample of the oil might be held for half an hour at the flash point temperature, without ignition, and then be poured off into a test tube and set away for 24 hours. It should then be examined for quantity and quality of carbonaceous matter and carbon present in suspension or settled out. Some oils form almost no carbon under this action; others will show a carbon deposit of one quarter the volume of the oil in the test tube. Probably these latter oils would be unsuitable for high temperature use.

The accumulation of carbon and carbonaceous matter may choke oil pipes and feed holes, and the carbon may act as a mild abrasive in bearings. The viscosity of oil tends to be slightly raised by the carbon; but the simultaneous formation of light hydrocarbons in the oil counterbalances this. The accumulation of this carbon is one of the reasons why the used oil should be periodically cleaned out of the crank case of an automobile engine. The oil which splashes up to the inside top of the pistons is spoiled on the hot metal, then drops back into the rest of the oil.

11. *Cold tests.*—As oils are cooled they increase rapidly in viscosity, that is, become stiff or sticky liquids. They may become so stiff that they will not flow at a readily measurable rate under the action of gravity. The temperature at which, in cooling, oil first gets too stiff for gravity flow is called the temperature of the pour test. Since gravity flow is very commonly relied upon to move the oil along feed pipes to bearings, cooling below the pour test temperature stops the feeding of the lubricant. On this account the pour test is of considerable importance as a low temperature limit to the use of the oil. Full force feed systems of supply of oil to bearings continue to work below the pour test temperature, down to or near to that temperature where the oil becomes practically a solid substance.

Between the pour test temperature and the solidifying temperature oils are, in appearance and lubricating action, what "greases" are at ordinary temperatures. Some "greases" are in fact merely oils with pour test temperatures above instead of below ordinary room temperatures. Some oils, of animal or vegetable origin, freeze as water does, at one temperature, changing suddenly from a thin liquid to a rigid solid. Most oils, however, become progressively more and more stiff or solid during cooling, with pour test temperatures somewhere between 40° and 0° F., and solidifying temperatures perhaps 40° or 50° F. lower still.

A cold test commonly made, but of no apparent practical application, is the cloud test. Many oils contain paraffin or similar waxes in solution, and in cooling throw this wax progressively out of solution as the temperature goes below a certain point. This point is that of the "cloud test," because the separation of the wax clouds the previously transparent oil. The cloud test is usually at a higher temperature than the pour test. The cloud test may be called the temperature of beginning of the partial solidification of the oil, which partial solidification may be in part responsible for the action (or lack of action) at the pour test temperature.

In many of the present automobile engines the oil

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[1]Assistant Professor in Experimental Engineering, Sibley College, Cornell University.

feed piping is internal to the engine. In such an engine even the pour test of the oil becomes of little consequence. If the oil is fluid enough to let the cold engine be started at all, the heat of friction will soon lessen the viscosity of the oil, and the chances are that the oil will be feeding properly by the time the engine will be ready to change from idling to appreciable power production. If the oil feed piping were external to the engine, not engine heat, it would behoove the user of the engine to be very careful in the winter time about the pour test temperature of the oil.

12. *Density.*—Density of oils is probably the property most often misunderstood and misinterpreted. Density is commonly measured and quoted in the oil trade in terms of an obsolescent scale, the scale of "Beaumé light" on a hydrometer. The relation between "Beaumé numbers" and ordinary densities, or specific gravities, with water as unity, is density = $\frac{140}{130+(\text{Beaumé number})}$. Thus Beaumé number 29.0 means a density of $\frac{140}{130+29}=\frac{140}{159}=0.880$. In strict accuracy, Beaumé number 10.0 corresponds to water as 1.000 at 60° F., where the real density is 0.998 on the scientific scale, which takes water as unity at its maximum density (at 4.0° C.)

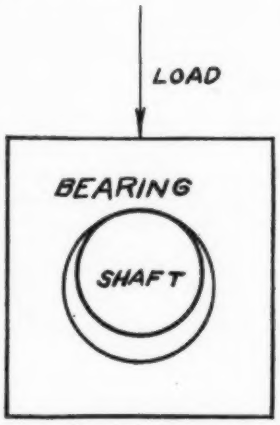

Fig. 1—Shaft at rest; load on

Density of oils has nothing to do with lubrication by the oils; lubrication does not depend in any way upon density as a specific property of the oil. When we speak of oils as "light," "medium," or "heavy," the property concerned is viscosity. "Light" means low viscosity; "heavy" means high viscosity. These technical usages of the words "light" and "heavy" are extremely unfortunate and highly misleading. They go back historically to a time when it was thought that oils of high viscosity were necessarily also of high density. That there is no such connection between the two properties may be readily brought out by citing liquids other than oils. Three common liquids have the properties at zero degrees Centigrade: water, density 1.00, viscosity, 0.018 c.g.s. units; mercury, density 13.60, viscosity, 0.017; glycerine, density 1.27, viscosity 46.0. It is true that oils made by the same process from the same crude oil have higher densities as they have higher viscosities; but change of process with the same crude oil as base slightly dislocates this relation of density and viscosity, and change of the crude oil entirely dislocates it.

What the density does tell concerning the oil seems to be the source of the oil—what kind of crude oil was used. Thus densities above 0.90 (Beaumé numbers less than 27) indicate in this country that Texas crude oil was used as base; densities below 0.90 (Beaumé numbers greater than 27) indicate Pennsylvania crude oil as base.

Density has an important commercial application when it is used to identify oils. Let it be supposed that a certain firm has by many trials found that one brand and grade of oil is for them the best for one use, saves the most power and gives the least cost of repairs to machinery and time out for repairs. (Cost of oil itself is less important than the value of power saving and repair saving). Before ordering more of this oil from the jobber the firm measures the density of the oil. If they find that the new shipment of supposedly the same oil has very closely the same density as the original, it is practically certain that they got what they ordered.

In the complete testing of oils density determination is necessary as an accessory to the common types (flow types) of viscometers. These instruments measure not viscosity but the ratio of viscosity to density, so that density must be known before viscosity can be computed.

13. *Viscosity.*—Viscosity is the most important property of oils as regards lubricating action. It is practically the controlling factor, so far as the oil is concerned, in the formation and maintenance of the oil film between metal parts when the parts are in motion. Viscosity is the internal friction of fluids, the force resisting the movement of one layer of oil past the next. A liquid which, like steam engine cylinder oil or tar, is "thick" or "stiff," and does not flow readily, has high viscosity; a liquid which is "thin," flows readily, like water, has a low viscosity. If two parallel metal surfaces of area A, distance x apart, have the space between them filled with oil of viscosity η, and move past each other with the velocity V, then the viscous force F opposing the motion is $F=\frac{A\,V\,\eta}{x}$, or viscous force = (area of oil film) (velocity difference at the two surfaces of the film) (viscosity) ÷ (thickness of film). Inverting this equation $\eta=\frac{F}{A}\,\frac{x}{V}$. With the forces, distances areas, and velocities in foot-pound-second or centimeter-gram-second units respectively, this equation tells the measurement. The numerical values quoted above for water, mercury, and glycerine are in c.g.s. units. The equation for viscous force is often written $F = A\eta(\frac{V}{x})$, in which the ratio $\frac{V}{x}$ is called the "velocity gradient."

When a shaft is at rest in a bearing, with load on, the shaft and bearing are in metallic contact on the side

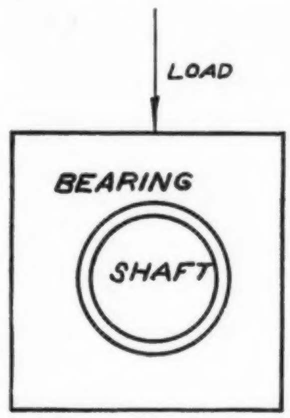

Fig. 2—Shaft running; oil film formed

toward the load. The shaft being smaller in diameter than the inside of the bearing, there remains a thin crescent-shaped space between shaft and bearing metal on the side away from the load. This space will fill with oil, while the shaft is at rest, by the action of surface tension or capillarity, even against the action of gravity. The relation of parts is as shown in Fig. 1.

When the shaft starts to turn, oil sticks to the shaft and tends to carry around with the shaft to form a film of oil between shaft and bearing on the side toward the load. The bearing, because of the load, tries to wipe this oil off the shaft, or to prevent the oil from going through. The oil layer in contact with the bearing metal stays stationary in any case. The result of the actions of the moving shaft and the stationary bearing metal is a motion of oil layer on oil layer, a motion which brings in the forces due to viscosity of the oil.

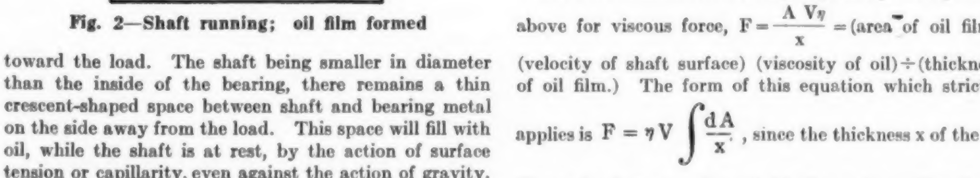

Fig. 3

The viscous force tries to push a wedge-shaped film of oil through between shaft and bearing. The wedge is a bent one, as shown in Fig. 1. The force carrying oil into the wedge increases both with the velocity of the shaft surface and with the viscosity of the oil, or is proportional to the product of velocity by viscosity. This viscous force has against the load on the bearing the mechanical advantage of the wedge, an advantage measured by the ratio of diameter of shaft to clearance of bearing (difference of diameter of shaft and bearing). When the viscous force, or product of viscosity by velocity, becomes large enough, the oil film rather suddenly carries through, and shaft and bearing take the relative positions shown in Fig. 2. The oil film must of course always be thinner on the load side than on the unloaded side, but with good lubrication the shaft does come appreciably near to running concentric with the bearing. In other words, the thickness of the oil film toward the load, where the film is thinnest, approaches as a limit, but does not reach, one-half the clearance. There is thus a mechanical limit to the thickness of the oil film.

Because of the relationships of forces just outlined, the thickness of the oil film toward the load in any given bearing depends upon the value of the fraction $(\frac{\text{viscosity x velocity}}{\text{load}})$. The change of thickness of oil film with change of this fraction is shown in Fig. 3. The curve OCEF has film thickness for its ordinates and values of the fraction $\frac{\text{viscosity x velocity}}{\text{load}}$ for its abscissas. OA is one-half the clearance, or the mechanical limit to the oil film thickness. The dash line HK lies at a distance BC above the base, and represents the heights of the roughnesses of the surfaces of bearing and shaft.

The course of events in lubrication during the starting up of machinery may be followed from Fig. 3. With velocity zero the fraction $(\frac{\text{viscosity x velocity}}{\text{load}})$ is zero, and there is no oil film (point O, Fig. 3). As velocity increases the fraction $(\frac{\text{viscosity x velocity}}{\text{load}})$ increases in proportion. The oil film builds slowly at first, and the bearing and shaft remain in partial metallic contact, with high friction. At some critical value of velocity the oil film becomes thicker than BC, Fig. 3, and metallic contact ceases. Thence onward to higher values of $(\frac{\text{viscosity x velocity}}{\text{load}})$ the friction of the bearing is entirely due to the oil. From C to E the oil film builds up rapidly; beyond E further change of the abscissas causes little increase of film thickness. Along CEF and beyond we are dealing with what is often called "perfect" lubrication. The term should more properly be "complete" lubrication. Between O and C lubrication is "imperfect" or better "incomplete." The formation of the oil film by viscosity and velocity action as described above is known as the "Tower effect."

The fluid friction of a bearing is given by the equation above for viscous force, $F=\frac{A\,V\eta}{x}$ = (area of oil film) (velocity of shaft surface) (viscosity of oil) ÷ (thickness of oil film.) The form of this equation which strictly applies is $F = \eta V \int \frac{dA}{x}$, since the thickness x of the oil film varies from one element dA of area to another, both because of roughnesses of surfaces and because the shaft is not concentric with the bearing. This equation, together with the relation shown in Fig. 3 governing thickness of oil film, gives the outline of the variations of fluid friction of bearings with changes of speed, load, and viscosity.

In the region from C to E, Fig. 3, thickness of oil film changes more rapidly than does the abscissa $(\frac{\text{viscosity x velocity}}{\text{load}})$. Putting this with $F = \frac{\eta V A}{x}$ shows that while the oil film thickness is rapidly increasing the fluid friction is decreasing, the factor x dominating the function. Beyond E and F, Fig. 3, the oil film has a practically constant thickness, set by the clearance of the bearing. In this region, then, since x cannot vary, increase of velocity or viscosity will increase the friction; but decrease of load will not decrease or increase the friction. Fluid friction in the ordinary bearing is not independent of load because the bearing is working in the region between C and F of Fig. 3, where change of load changes the film thickness. It is possible also that increase of load, and hence pressure in the oil film, causes an increase of viscosity of the oil; but such an effect remains yet to be demonstrated as of practical importance. The seizing of a bearing due to pressure

application comes from squeezing the oil film down till metallic contact occurs.

After a machine is under way the friction causes the film and the bearings to warm up. This complicates the problem, because viscosity of oils decreases rapidly as temperature increases, especially at lower temperatures. The decrease of viscosity decreases the fraction ($\frac{\text{viscosity x velocity}}{\text{load}}$), velocity and load being now constant, and may affect the film thickness. The viscosity decrease may go so far as to change the film thickness from F to E to C, Fig. 3, allowing metallic contact and seizing of the bearing. If such seizing occurs repeatedly during the operation of a machine, it indicates that an oil of higher viscosity is needed.

Excessive friction in machinery may occur therefore from either too low or too high a viscosity of the oil. Too low viscosity means that the oil film is not maintained. Too high viscosity, while the oil film is "perfect," gives excessive friction in the oil itself. Anyone who has cranked an automobile engine in the winter time appreciates the possibility of having too much viscosity in the oil. Too low a viscosity, with seized bearings, wrecks the machinery; too high a viscosity causes no damage to bearings, but is inefficient. The highest efficiency comes from going as near to seizing as we dare, while still playing safe.

Because viscosity varies so rapidly with temperature, and the variation is at different rates in different oils, we need in testing the oil for viscosity to find the viscosity as a function of temperature. A knowledge of the viscosity value at one temperature is not sufficient. What we need most to know is the viscosity of the oil at the temperature at which it must operate with the most severe running conditions of the machine in which the oil is used. We must be sure of having enough viscosity to maintain the oil film under the extreme conditions of operation. Then we put up with the higher viscosities when the oil is colder. Usually the range of temperature through which the oil must lubricate is narrow enough so that we can have both enough viscosity at the high temperatures and not too much at the low temperatures. In extreme cases, as the operation of an automobile engine in the winter time, we cannot get enough viscosity left at high temperatures without getting excessive viscosities at starting of the cold engine. The range of temperatures is too wide, considering the unavoidable and uncontrollably rapid change of viscosity with temperature. The ideal would be an oil which did not change viscosity with temperature.

It can readily be shown that the fluid friction of a roughed surfaced bearing is greater than that of a smooth one, other things being the same. From $F = \eta V \int \frac{dA}{x}$, with η and V the same in the two cases, F varies as $\int \frac{dA}{x}$. Let the total area in each case be 2, and the average film thickness be 2. In the rough one let the two halves of the film area have thickness of 1 and 3; in the smooth one 2 and 2. Then $\int \frac{dA}{x}$ for the rough one will be $(1/1+1/3) = 1.33$; and for the smooth one $(1/2+1/2) = 1.00$. It should be said, however, that in practice the smoother bearing would probably be made with less clearance than the rougher; and in turn the oil used in the smoother bearing would be of lower viscosity. The change in the automobile engine from hand scraped bearings to reamed bearings will probably bring about all these changes in succession.

The Geology of West Africa*

With Notes on Its Resources and Future Development

By R. Chudeau, Director of the Expedition in French West Africa

THE Geology of Occidental Africa has already been made the subject of numerous works, particularly abundant during the past fifteen years. Many attempts at synthesis have been made. The already old-fashioned studies of Pomel in 1872, and of Du Ponchel in 1879, which were based on too small a number of precise observations, are today of but little interest. In 1891, G. Rolland, who had taken part in the labors of the Choisy Expedition, published a Geological map of the Sahara; it has been reproduced with some modifications in the "FASCE de la TERRE" (Face of the Earth) (Vol. 1, p. 459, 1897) for the central part of the Sahara and the northern of the Soudan, I myself made in 1897 an attempt at a map, a corrected reproduction of which was published by E. Suess in 1911 ("LA FACE de la Terre" (Vol. 3, p. 672). In 1913, subsequent to the expedition for the study of Trans-African railroads, I was able to make certain additions to this document (Bull. Soc. Geol. fr. 4, x 111, p. 173).

All the attempts at maps referred to above are on a small scale. In 1909 H. Hubert published a color map of West Africa on the scale of 1:5,000,000; although this document is already out of date, it is still very useful and sufficiently indicates the main lines of African Geology. More recently, P. Lemoine[1] has made an extensive bibliographical list (333), and a table of twenty-two geological maps relative to West Africa; he has made a résumé of the results obtained. Since then more new notes have appeared, and an analysis of the majority of them will be found in the: *Annuaire et Mémoire du Comité d'Etudes Historique et Scientifique de l'Afrique Occidentale* (Gorée, 1916).

The greater part of the soil of West Africa and of the Sahara consist of granitic massifs and crystalline schists. The first (Granite and Granitoid, Geneiss) sometimes gives birth to peneplaines, and more often to granitic chaos, or regions of domes. The regions of granitic chaos are formed by the crumbling of blocks, whose size varies from that of the human head to several cubic meters. In the regions of domes the granite is always porphyoid, with large crystals of Feldspaths; these domes are merely blocks of very large dimensions, of which the portion disengaged from the earth often exceeds 100 meters. This structure of the granitic regions is found throughout the world; it is connected with the nature of the granite which, when in the pasty state formed itself, like most viscuous bodies, into harder lumps of varying dimensions. Erosion confined itself to the removal of the least resistant portions, intercalated between the lumps. The aspect of the granitic regions varies with the altitude. In the Sahara, by reason of dryness, vegetation and vegetable earth are rare.

The areas of the granitic chaos present only piled up blocks, which from a distance take on the appearance of mountains, though the length may be only a few meters; analogous landscapes are known upon the Breton Literal where the vegetation is sparse. Further to the south the vegetable earth fills all the interstices between the blocks, and during the season of rains, in the midst of the verdure, only a few rounded boulders of granite are to be seen, quite as in the Limousin. As for the granitic domes, their steep walls are everywhere denuded. In the Sahara, as well as in Dahomey, on the Ivory Coast, or in the Vosges, the crystalline schists form a complex ensemble, which certainly represents several stages; in Southern Africa, is found their analogue (pre-Cape formation) which the more complete study of the country enables us to divide into four series. We are less far advanced in Africa where we are as yet able to introduce only two subdivisions, according to whether the schists or the banded gneiss predominates. This classification based on the nature of the rocks is of only mediocre value and may betray a difference in the intensity of the metaphorphism quite as easily in the age of the strata.

All these schists exhibit strata which are much tilted, often almost vertical; the out-crops often run nearly north-south along the direction of the meridian. This direction, as we know, plays a part of the first order in the structure of the African earth crust; the great breaks which have given birth to the lakes of East Africa and to the Red Sea are parallel to it.

The crystalline schists offer poor resistance to external forces, they rarely form peneplains, and sometimes true plains, when the phyllades or schists marked by sericity predominate. A few reefs which have been observed there are due to eruptive rocks, especially to the diabases, and are then not strongly marked; or less to quartzites which form long crests towering above the neighboring peneplains (the Atacore chain in Dahomy, + 300 meters; Belia chain near Hombori, + 100 meters etc.).

The age of the crystallophyllian territories is everywhere difficult to fix; in the Sahara it can be affirmed that they are previous to the devonian and even to the upper silurian period; in the Soudan, it is believed, merely because of their geographic continuity and, therefore, without sufficient proof, that they are of the same age. These territories are often covered in Africa by mighty masses of sandstone in horizontal layers. These sandstones have grains of very various size and pass through every degree from real pudding-stone to very fine sandstones. They are generally light in color, but covered with a deeper patina. In the Sahara this patina, which is called the desert varnish, is extremely hard and as black as pitch.

These sandstones form three principal bands; the first extends from the northern part of the Ahaggar through Ouallan, as far as the vicinity of Mourzouk, over about 800 kilometers from West to East (Ahnet Mouidir Tassili of the Ajjers), and fossils are quite abundant there, and it has been known since the voyages of Overweg (1852), and especially since those of H. Duveyrier (1860), that these plateaus are Devonian. The most recent researches have shown lower Devonian, especially well developed there; the Middle and Upper Devonians are represented towards the north of these plateaus by the clays and the lime-stones. Quite recently M. Doubillé has shown that the basis of these plateaus belongs to the Upper Silurian.

The second band, more discontinuous, begins in Mauretania (Adrar, Tagaut) and reappears around the Adrar of the Iforas and rejoins at Aïr. It is probable that this also belongs to the Devonian age. In the Adrar of Mauretania and in the neighborhood of the Aïr the sandstone, which constitute, it is covered at certain points by lime-stones whose fossils permit us to attribute them, without hesitation to the Carboniferous.

As to the third band, it begins in the vicinity of Kayes and extends as far as Hombori, covering a surface of 300,000 kilometers. There is nothing which permits us to connect it with the Devonian except analogy of aspect and a geographic semi-continuity.

Other sandstone plateaus are found in Guinea, in the region of the Tchad (Tibesti and Enedi), without our having any better knowledge as to the period of their formation.[3] Whatever be their age these plateaus everywhere play the same rôle in topography and in geography. From the topographic point of view these considerably cut-up plateaus often display abrupt cliffs and the aspect of ruined cities, such as we find in the grit-stone Vosges, everywhere in the Sahara because of the dryness of the air. Their sides bear no vegetation, and are more denuded than those of the European lime-stone plateaus. Certain gorges form remarkable landscapes, like those of Takounbaret) Mouïdir, which remind one of the cañons of the Caucasus. The sandstones of the Sahara plateau are covered with a black patina of almost metallic brilliance, quite unknown in Europe. Further south the aspect is more normal.

From an hydraulic point of view these sandstones are of the highest importance. Thanks to their permeability the Saharan Tassili (plateaus) all contain numerous pools of water, frequently quite shallow; sometimes, even, in certain valleys, the water forms little ponds which are almost permanent (Tagergera, Tahount-Arak). In the Soudan wells are frequent at the foot of the plateaus; there are even a few villages built on top of them which possess shallow wells. At some points springs are known. These sandstones are hard and resistant; with the primitive means at their disposal the natives have been able to extricate only a small portion of the reserves of water which they contain.

The marine carboniferous is widely represented in the Sahara by fossilferous limestones; there are but few of these deposits in Occidental Africa: There is an outcrop from East to West in the Southern part of the Mauritanian Adrar; another constitutes the Hamada el Haricha near Taodeni; there-is a third, finally, between the Adrar of the Iforas and the Aïr.[4]

*From *Revue Scientifique*.

[1]P. Lemoine, Occidental, Africa. In Handbook of Regional Geology, Heidelberg, 1913.

[2]The quartzites alone form an exception; they occupy too restricted an area to play an important rôle.

[3]Further to the East the age of the Nubian graves displays an analogous lack of definite character.

[4]A coal basin is known in the South Oranais, near Colomb Béchard; in the region of the R at and in the erg of Isonian, Overweg and Fourean have collected some remains of vegetion. Up to the present time, in the Sahara of the South the carboniferous is purely marine.

Farther South, along the road from Kayes to theNiger between Dinguira et Toukoto, a rather ambiguous formation appears. It occupies a portion of the depressions which extend between the sandstone plateaus, although it is lower than the sandstones upon which it rests in discomformity. At the base are found the limestones made use of for burning lime at Dinguira et Toukoto. Above come the Schistous Clays loaded with silicia, often replaced by banks of jasper, sometimes by blood-stone. Some of the latter may prove to be exploitable.

Leaving to one side this ambiguous formation the first terrain encountered in Occidental Africa after the Carboniferous belong to the Lower Cretaceous. They are very well devloped in the northern part of the Soudan, and in the Sahara they form around the ancient central massif a vast ring whose northern border touches the southern line of Algeria. They habitually give birth to plains whose altitude varies from 200 to 500 meters. The valleys and the Thalwegs there are generally slightly marked, and are often transformed into groups of ponds. Certain clayey regions would be marshes in our climates; even in the Sahara some of them are difficult to cross when it has rained. The most typical one of these plains is the Tegama Zinder et Agadés. This lower Cretaceous is formed of gravels and clays; the only fossils there known are the remains of great reptiles, and of silicified forests (Conifers). Waterworn fragments of these conifers are found everywhere in small quantities in the gravels and in the clays, and at certain points which were ancient estuaries they become abundant and sometimes attain great dimensions. At Taourirt (Touat), at Tit (Tidikelt) and at Marandet (Tegama) are found trunks of tree several meters long. Reptiles, which are less widely found, are, however, frequent in the Soudan; Marandet is thus far the only deposit where these remains are abundant and of notable size. These Fossils, though indicating the Lower Cretaceous, have not yet afforded any specimens which is truly determinable; but in many regions the gravels and the clays are covered in conformity by the Fossiliferous Cenomanian. They habitually repose upon ancient primary or crystalline terrains. However, in the extreme northern part of the Sahara, in the Djebel-Amour, they rest upon the aptian. In this region they are albian; but it would be imprudent to extend so strictly limited a denomination to the ensemble of this formation as far as the Soudan; it is better to content ourselves with classing it in the Lower Cretaceous without seeking to locate it too definitely.

The seas of the Middle Cretaceous have penetrated Africa and extended from the Tripolitaine to the Cameroun, in the Soudan. They left very clearly marked traces (clay and limestone) in the Tegama and the neighboring regions. Farther to the West, in the basin of Bamba, only sandstones are to be seen at this level. It is not until we come to the Upper Cretaceous that a fault enables the sea to penetrate freely towards the West, running along Agrar of the Iforas, and extending towards the north as far as the vicinity of Ahnet. Although the Cretaceous appears to be complete since the Cenomanian, it is not yet possible to indicate with precision the extent of the various stages which compose it in the Soudan. Characteristic Fossils from a certain number of levels have been obtained, and up to the present time no inconformity has been pointed out in this ensemble. But the researches upon this terrain are still insufficient to permit the elucidation of all the details.

These various stages of the Cretaceous are principally formed of oyster clays, sometimes with Ammonites (*Vascoceras* especially in the *Turonian of the Damergue*) and marls and limestones enclosing corals, and especially molds of bivalves (*Eligmus, Roudairea*) and Gastropods (Adrar Tiguirrirt, *Adrar of Tahoura*), as well as sea-urchins. In the proximity of the ancient massifs the clayey gravels sometimes replace the clays or the marls.

The limestones in the vicinity of European colonies have sometimes been exploited for the manufacture of lime, at Tamaské for example, in the region of Tahoua. Unfortunately none of these limestones has been found within reach of a good route of communication, such as the Niger; hence the manufacture of lime is only a local interest.

The seas of cretaceous were shallow in the Soudan; they readily form lagoons which have left as their trace deposits of gypsum (Damergou, Tiguirirt, etc.). These deposits are of too little importance and too far from the river to be worth serious exploitation.

In the cretaceous regions, where the limestone plays a subordinate role, and where the clays predominate, the usual geographic form consists of rounded hills (Damergou); when the limestones assume more importance plateaus predominate. They are cut by wide, deep valleys called "Dallols" (a Peul word, meaning water-course), above which tower tall cliffs, sometimes as much as 100 meters high. These Dallols, the principal ones of which run nearly north and south, can be followed for great distances; they are the last traces of rivers which descended from the heights of the Sahara in the Quarternary period, and flowed into the Niger. The most typical of these plateau regions are the Adrar of Tiguirrirt and the Adrar of Tahoura.[5]

Another important cliff involves the Cretaceous; it runs nearly East to West in the vicinity of 18° North latitude, and extends very nearly 800 kilometers from the Tilemsi to beyond Agadés. In this eastern portion it is cut into the gravels and the clays of the Lower Cretaceous further on into the strata of the Middle Cretaceous; still further west into the limestones of the Upper Cretaceous. The erosion alone does not suffice to explain this. A study of the magnetic declination shows that it corresponds to a zone of anomaly which, according to our knowledge of the same thing in Europe, must have been caused by profound tectonic accidents. Starting from Teleusi, the long cliff is folded towards the North; but it changes its character and becomes merely a crest flanking at the west the mountainous massif of the Adrar of the Iforas.

Marine Tertiary terrains are found in Lower Dahomey, and at some points of the littoral of the Gulf of Guinea. They are well developed in Senegal, where the sea has penetrated in a rather deep gulf, which reaches the *Falémé*. The *Lutaceous Eocene* is especially found there, well characterized by nummulites and a few molluscs. In the neighborhood of the ancient littoral the gravels predominate; they are soft sandstones, light in color with red spots; as one proceeds these gravels give place by degrees to clays enclosing calcareous lenticular bodies.

Apart from the nummulites few of the fossils found in the Eocene of Senegal are very characteristic. In general the Molluscs are similar to those of the Upper Cretaceous. This similarity of formations explains why the most elevated strata of the regions of the Bamba and of Tahoura have sometimes been classified in the Eocene, and sometimes in the Upper Cretaceous. Even in Europe the boundary line between the secondary and the tertiary strata is not always easy to define when neither Gastropods nor Lamelli branches occur. Recent studies made by H. Douvillé have proved that the upper terms of the Soudan belong decidedly to the Eocene; some of the most distinct of their fossils are found in Senegal in the same banks as nummulites.

We have, as yet, few indications concerning terrains more recent than the Eocene in Africa. Even at Dakar *Lepidocyclina* limestones belong to the Aquitanian; in the Eastern part of the Adrar of Tahoura (a few molluscs), have been found in the *Laterites* of Boutoutou, which indicate the Miocene. Schistous clays containing fossils of plants, covered by marine layers, are found in the latitude of Boutoutou, a proof that the sea did not retire from these regions until towards the end of the Miocene.

The quaternary formations are rather widespread in Eastern Africa, as they are everywhere. Marine formations of this period are found in the coastal plains particularly in Senegal and in Mauretania where they indicate the existence of a deep gulf which has been recently filled in. The most interesting of these are terrains lying at different altitudes up to 40 meters in height which show the comparatively recent changes of level in the Atlantic Ocean.

In the great valleys the quartenary formations are well developed, but are not really as interesting as they are in the northern part of the Soudan and in the Sahara. They contain numerous molluscs: the terrestrial fauna (principally, *Helix*) of Mediterranean affinities have hardly been able to penetrate as far as the vicinity of the Soudan. The fresh water fauna, on the contrary have their principal affinities with Tropical Africa. During the Quarternary, the Ethiopian river fauna was able to make conquest of Minor Africa; a few rare species (*Melanopsis*) belonging to Moroccan groups have been able to travel even into the Igidi. We thus have a new proof that during the Quarternary the hydrographic systems of the Sahara and of the Soudan were probably connected with each other; this view is also supported by the geographical distribution of fishes, batrachians and certain plants.

Other types of quaternary formation are of more interest to us. From Cape Blanc, and particularly from Cape Timiris, as far as the vicinity of Daka, the Atlantic is bordered by littoral dunes, behind which are found numerous ditches which in Mauritania are salty and often dry (Sebkha), and in Senegal are filled with fresh water (Seyanes). This is the same arrangement as in the Landes; they are of no special interest. It should be noted, however, that from Maroc to Daka the trade winds slightly deviate, come almost from the north and follow the Littoral. The dunes run toward the south, and cover the northern shore with sand, particularly at the mouths of the rivers, which thus find themselves thrown towards the south. The Senegal and the Langue of Barbary furnish a definite example of this fact.

The continental dunes are more interesting; we know what an important role they play in the Sahara, of which they cover perhaps a tenth part; they form there numerous massifs, called *ergs*, the most important of which, found in barren parts of the desert, correspond in a general manner to the great plains of low river valleys, which are today entirely dry. The ergs form, as a whole, a rough sketch of the ancient hydrographic system, the sand which forms them is a sand of alluvial earth, freed of clayey dusts, refashioned, and heaped in piles by the wind. Only the gravels and the pebbles remain in place on the ground, forming great pebbly plains called regs, which are very characteristic of the Saharan landscape. The Saharan dunes are deformed under the influence of the wind, but they are but slightly displaced; in order for the sand to accumulate topographical conditions are required which are very definite; but which are, as yet, but little known. Only the sand itself is mobile. Ananogous conditions are to be found in the clouds, which persist in hanging around certain mountains for a long time in spite of the wind; the water vapor moves constantly, but does not become condensed except around the summit of the mountain. To prove the great mobility of the dunes some authorities have often cited the actual burying by sand of villages and oases, without reflecting that the planting of palm trees and the building of houses created the obstacles which supported the sand.

The Saharan dunes show very varied arrangements. The growing dunes, the *Barkhane*, which is often mentioned as the typical form of the dune, is rare, and appears to be, on the contrary, an accidental form. The most frequent arrangements are the two following:

Sometimes the sand outlines a series of hummocks and of hollows in whose ensemble no order can be distinguished, only a few crests of short length have a definite direction. These confused masses are of variable extent; they are usually only a few meters in height; occasionally they accidentally attain, and sometimes exceed, 100 meters. When the dunes attain so high a relief as this they always have a rocky substratum; they are then really only sand-covered plateaus or hills; very often fragments of rock emerging from the sand indicate that such is, in fact, their origin.

Very often, also, the dunes present themselves under the aspect of long *cordons*, parallel to, and not perpendicular to the direction of the wind; these cordons can sometimes be followed for hundreds of kilometers; their width is slight, rarely attaining 100 meters, and it is sometimes less than one decameter. They are usually 5 or 6 meters in height with a few summits of 25 meters overlooking the plain here and there. These long ribbons of sand are usually grouped in series of 5 or 6, occupying a band of a few kilometers; they are sometimes united by inosculation. North of Araouan, for example, one has to cross a dozen of these groups in a distance of 150 kilometers.

The mode of formation of dunes still presents numerous obstacles; their form is evidently that which presented the least resistance to the displacement of the air (a solid of less resistance); and this explains the analogy often pointed out between the forms of fishes or of the keels of boats with certain details of the dunes. But too many elements are still lacking to enable us to treat seriously this problem of hydrodynamics or of aerodynamics.

The Saharan dunes bear little vegetation and the slightest imprints remain legible on the mobile surface of the dune; for example, the trail of insects is easy to follow. The winds cover them with elementary ripples and certain photographs of dunes recall very exactly, the rippled surface of water.

The southern boundary of these dunes coincides with that of the Sahara; it leaves the Atlantic at Cape Timirik, 19° and 22′ N. L., and runs to 15° north of the Tchad. It is not a straight line, but runs around a number of elevated massifs to the north of the Adrar of the Iforas, it reaches in Ouzel (20°-42′). These dunes of the Sahara, therefore, scarcely involve East Africa. Further to the south, from the Atlantic to Ouadai, and even beyond, numerous massifs of dunes are found which, as in the Sahara, may either present confused masses or be definitely oriented.

These dunes are fixed by spontaneous vegetation; they have lost their live bases, their crests are softened and their forms more flattened. The study of their transformation, which is easy to follow from the Sahara to Senegal, shows that they are derived from the dunes of

[5] In the Touarg tongue there are two distinct word: Adrar, meaning a mountain chain, and Adr' ar, a country of pasture lands: it has become the habit in Europe to confuse these, as in the usage of the Arabs. Tahona and Tiguirirt belong to the Adr'ar's, as they are correctly termed on certain maps, as Barth has indicated.

the desert under the influence of the rain, which has softened their forms and permitted vegetation to establish itself. The rain has had yet another action. The clayey dusts, which are much more mobile than the sand, have disappeared from the Saharan dunes; the wind has blown them afar, often even to the Atlantic. In the Soudan these dusts have been constantly carried down to the surface of the ground by the rain, and made to penetrate the dunes, causing their sand to acquire a clayey texture once more.

These dunes of the Soudan are, in reality, fossil dunes; they could not have been formed in the present climate, which is too rainy for their creation. They had their birth in a climate like that of the Sahara, and they furnish a proof of the change of climate, all to the advantage of the Soudan. It is well to emphasize somewhat this question of dead dunes, and of the change of climate of which they are the proof. A whole school of geographers, particularly the followers of Prince Kropotkin, have believed that the earth is continually growing dryer, and that everywhere the deserts are encroaching, at times very rapidly, upon the more fertile neighboring regions. A more careful examination of the facts cited has proved the inexactitude of this hypothesis, which rested upon observations which were poorly made and often upon a confusion of dates[6].

It is well known that after the glacial epoch in Europe there was a dry period during which the fauna of the Steppes of the Caspian extended itself as far as Spain. It was not until later that the present régime of forests was established in Europe. But these changes are ancient, and since the beginning of historic times, since the Greek Epoch, we possess proof that the climate of the Mediterranean has not varied appreciably; nowhere has any evidence of any change of climate been offered in China any more than in Europe.

It would be difficult to comprehend how there could be a profound modification of the climate of the Sahara and of the Soudan co-existent with the fixity of the Mediterranean climate; the fossil dunes could only have been formed during the ancient quarternary, before the establishment of the forest climate in Europe. For the lack of local paleontological arguments it is not possible, at present, to seek a more precise definition. In a general manner the contrast between the live dunes of the Sahara and the fossil dunes of the Soudan is considerable, as much so as between the two climates. However, for causes which are in some sort accidental, even outside the littoral these dunes may still have their birth in the present time in the region of the dead Ergs. In the bed of the Niger there exist extensive beaches which are often uncovered at times of low water; the extremely fine materials of which they are made are refashioned by the wind, and one is able to see on the borders of the river some partly denuded dunes which recall from a distance those of the desert. In Senegal certain dunes where the peanut is cultivated are deprived of all vegetation at harvest time; their soil, broken up by harrows, gives an easy hold to the wind[7]. Until the next winter these dunes present vague analogies with those of the Sahara; an example of this can be seen near Thiés; but these few facts in no way prove that the climate of Africa is deteriorating. Some other facts cited as a proof that the Sahara is growing in extent at present towards the South have not the significance which has been attributed to them. It is undoubtedly the fact that the last few years have been comparatively dy in Europe, as well as in Africa; but this fact is explained by Brocker's Law of the short oscillations of climate, which have been made evident by the study of glaciers and by many other phenomena. They prove nothing as regards a secular variation.

The dunes of the Soudan are of great importance from the agricultural point of view; when it rains the water accumulates there, and under the protection of the sand remains much longer than in the nearby winter ponds. From Senegal to the Tchad the sand-covered depressions, very slightly marked—as a rule, form excellent land for the cultivation of peanuts and cereals (millet and sorghum), sometimes the layer of damp soil is thick enough for the digging of cisterns, which are almost permanent.

Lateritic products are characteristic of tropical countries. In the primitive sense of the word, the true laterites result from the decay of various volcanic rocks, and it is by a real abuse of language that in the Soudan people have fallen into the bad habit of designation by this name all superficial formations having a red color.

Laterite in the strict sense of the word is a definitely localized product, which in Africa is scarcely found North of 13° N. L. In place of being transformed under the influence of external dynamics into clayey products (hydrated silicate of alumina), as in our climate, the volcanic rock has lost its silica, and naturally also those of its elements which are readily soluble, the alkaline metals and the alkaline earths; nothing remains except hydrates of alumina and iron, i. e., a bauxite. The transformation is abrupt, and under the crusts of laterite, the unaltered rockis found without any transition stage. The transformation into clay is ordinarily more progressive. The formation of laterite is still obscure; it is evidently in part connected with climate, but there must be other causes also; sometimes two rocks of the same nature, and situated near each other, are transformed one into laterite, the other into clay.

It has been supposed that laterization was the work of microbes; a map of the distribution of laterites recalls, in fact, the geographic distribution of an animal or a plant; a central optimum zone, in which laterization is general is surrounded by regions where the laterization occurs only in spots whose number and importance constantly diminish. The limits are fixed by the lowest temperatures rather than by the average temperatures. This mode of geographic distribution is, up to the present, the sole argument which can be cited in favor of the microbe hypotheses; the biochemical mechanism has not been explained, and no microbic organism has been put in evidence. Some authorities have cited also the richness in nitric acid of the tropical rains, and the role of the termites which, destroying vegetable debris, deprive the soil of humus and form a material which is oxidizing rather than reducing. Up to the present time all of these hypotheses are mere possibilities.

Laterite presents itself under various aspects; it forms a scoriaceous crust, full of bubbles and looking as if it were varnished, which has sometimes been mistaken for a flow of lava; it often presents a pisolithic structure, and contains numerous concretions which may occupy the place of the rock from which it comes; in these laterites "in place" the structure of the ordinary rock is sometimes preserved. It may also have been carried away by the trickling of water and form "refashioned" laterites, where, in the midst of a lateritic cement, one finds galets or clayey pebbles from various sources.

The gold of the Soudan is often found in these laterites; but portions of it are rich in iron particularly, and may be regarded as actual minerals. The natives formerly made much use of these, and still exploit them at certain places, which the difficulty of transport renders not very accessible to European products. Others are aluminum minerals, poor in iron and in silica.

Volcanoes are comparatively numerous in Africa; there are several in existence in the vicinity of the littoral, between Dakas and the Cameroons, as well as in the islands of the Gulf of Guinea. They have been found also in the interior of the continent at great distances from oceanic masses; nearly all are found in mountainous regions; they are numerous in the Ahaggar and its annexes and in the Air; they have also been found in the Tibesti.

A group of three or four vo canoes erects itself in the lowest part of the Sahara in the vicinity of the celebrated salt deposit of Taou denni (alt. 150 meters; at 660 kilometers north of Timbuctoo). This little group is connected with the sunken basin of Djouf which bounds on the south the long cliff of R'na chich.

The lavas from these various volcanoes are of diverse types; many of them may be classed among the alkaline types (rocks which are poor in lime and rich in alkaline metals) a group which is rather poorly represented in Europe. Granular rocks of the same chemical character are found in West Africa and in the Sahara. They define two petrographic domains. The western of these comprises Dakar Senoudebou (on the Falemé) and the islands of Loes as well as the archipelagoes of the Azores of the Canaries and of Cape Verde. The central province contains Ahaggar, the Air, Zinder the Mounio the southern portion of the Tchad and the Cameroon.

The age of these various rocks is not well known. At Dakar the volcanic discharges have had their beginning in the Upper Cretaceous, a certain number of alkaline granite appear to date from the same period. But the majority of the lavas have made their appearance during the tertiary (subsequent to the Upper Miocene) and the Quarternary.

The mineral resources of West Africa are not very considerable so far as is known at present. The salt deposits of the Sahara (Idjil in Mauritania, Taoudini, in the northern part of Timbuctoo, to mention only the chief ones) have furnished by means of slaves great quantities of salt to the Soudan; since the suppression of this slave trade this commerce has fallen off. The development of railroads gives the final blow to this ancient traffic by enabling salt from Europe or the African littoral to reach the interior of the continent.

The gold of the Falemé of the Upper Niger, of the Gold Coast has always been made use of by the natives; a few European companies have launched enterprises along this line, too often without sufficient investigation.

The iron minerals, well known to the native blacksmiths and those of aluminum and manganese are abundant.

We must remember, finally, that Africa is still far from well known and that many surprises are possible. In very recent years an important coal basin has been discovered in the Cretaceous deposits of southern Nigeria, and its exploitation has been already begun.

Moreover, West Africa is rich enough in its agricultural and stock raising possibilities to have little need of mineral resources; its future does not depend upon the latter.

[6]Fd. Herbette. The Problem of the Drying Out of Interior Asia. (Ann. de Geog., xxii, Jan. 15, 1914, p. 1-30.) A Woeikof. Russian Turkestan, Chap. viii, p. 107-116, Paris, 1914.

[7]The carpet of vegetation which covers the dead dunes which are not cultivated consists chiefly of perennials; the top portions partially disappear during the dry season, but the roots persist and hold the soil in positions. The clay which accompanies the sand of the dead dunes forms the cement of a soft sand stone which aids the roots to protect the surface of the dune.

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